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A

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BRITISH AND FOREIGN

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JANUARY, 1853.

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Chronicle of Medical Science.

NEW FEATURE

IN

THE BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

NOTICE.

SOME changes have been introduced into the current Number of the Review, which demand a few words of explanation.

The names of the reviewers are, in several instances, affixed to the reviews. It is hoped, by this plan, to make the criticisms even more valuable than formerly—since the reviewer will appeal more authoritatively to his own experience. A review is simply an inquiry into the validity of an author's statement by the aid of previously-admitted facts, or by the assistance of others known only to the reviewer as the result of his proper observations. To give these last their due weight, they must be authenticated. A writer who merely builds up his argument with well-known truths, or with the opinions of other men, may remain anonymous; but whenever he resorts to his own experience, he must guarantee it with his name. An unavowed statement is dead to science, and no one recognises a veiled authority.

By far the most valuable kind of criticism is that in which a reviewer starts from the same point as his author, and treading in his footsteps, explores afresh the same country, confirms what is true, and corrects what is inaccurate. We have seen this mode of reviewing carried in this country to as high a degree of perfection as the employment of the incognito can ever permit. It remains to be seen whether it may not be made still more authoritative, and

therefore more valuable; whether, in fact, it may not be possible to permit no statement to pass into the currency of general belief until a second name has been affixed to it, as a guarantee that the evidence on which it is made has been tried, and not found wanting.

By the adoption of this plan, also, it will be possible to bring more frequently before the profession the matured views of experienced men in various departments of medicine. At present, such reviews lose much of their weight, because their authors are unknown, and they are less numerous than they might be, since no one wishes to put the labour of years into an article which carries with it no evidence of its source.

It is not intended, however, to lay aside altogether the incognito. In some cases the addition of the name may, perhaps, be unnecessary, in others inexpedient. Experience alone can show how far the interests of truth and science may demand publicity, or warrant secrecy.

In order to maintain the high character of the reviews, the aid of the most competent men will be sought; and no assistance that is worthy and honest will be disdained. This journal has never been, and will never be, the organ of a section or a party; it is a catholic work, in which all who love their science will be invited to join.

Some Original Communications are now, for the first time, introduced. A new and advantageous medium of publication is thus afforded, and the journal itself will gain in variety, interest, and utility. Occasionally, it is intended, also, to translate foreign papers of peculiar merit, of which an example appears in the present number.

The term "Chronicle" has been substituted for that of "Periscope," as expressing more fully the intention of this part of the Review, which is, to record as concisely, though as faithfully, as possible, the progress of Medical Science.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1853.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Handbuch der Pathologie und Therapie.* Von Dr. C. A. Wunderlich, &c. In drei Bänden. Zweiter Band. Erste Abtheilung. Die Anomalie der Constitution. Elfte Lieferung.—*Stuttgart*, 1852.
Manual of Pathology and Therapeutics. By Dr. C. A. Wunderlich. In Three Volumes. Vol. 2, part i. 'Constitutional Affections.'

2. *A Treatise on the Practice of Medicine.* By George B. Wood, M.D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania; President of the College of Physicians of Philadelphia; one of the Physicians of the Pennsylvania Hospital; one of the Authors of the 'Dispensatory of the United States of America,' &c. &c. Third Edition. Two Volumes.—*Philadelphia*, 1852.

Treatises on the Practice of Medicine are inevitably voluminous, whatever may be the method adopted by the writers. The perfect freedom of opinion enjoyed by the profession renders it necessary to appeal to the judgment of the reader, and the statement of premises and grounds of opinion inevitably involves more or less of theory or of questionable observation. It is not surprising, then, that Dr. Wood's treatise is spread over seventeen hundred closely-printed pages, royal octavo, or that Wunderlich's Manual, to be published in three thick volumes, is only half completed, although the work has been three years in progress. If the reader will glance round his library, he will see that voluminousness is the characteristic of all the systematic writers, for the shelves groan beneath the weight of modern thick octavos, or less modern quartos, or older folios. A refuge from the evil has been sought in 'Libraries,' or 'Cyclopædias,' or 'Dictionaries,' but sought in vain, for these are more voluminous still. 'Conspectuses' and 'Manuals' have been tried, and have met with some success; but the generalities of a *conspectus* are too abstract for any than the most abstract mind, and the dry summaries

of a manual too dogmatic and general for the practitioner, unless edited with masterly tact.

There is another difficulty the reader has to encounter, of no little importance. Having selected his standard works of reference, they quickly depreciate in value, because, with every year, they less and less represent the existing state of medical science and art. The science of life is too little elaborated to supply fixed general principles of pathology and therapeutics, and the practitioner must be content to gather from the medical periodicals, from discussions at medical societies, and from the perusal of systematic treatises, as they issue from the press, that knowledge which it is absolutely essential he should possess, if he would keep well forward in the race of life It is not the "portentosa morborum phalanx' which he has alone to encounter; the sharp rivalry and intellectual competition of his brethren is no slight difficulty in his way, and no unimportant stimulus (fortunately) to his efforts; not to mention those less important, but not less troublesome, annoyances and injuries inflicted upon him by empirics. The medical practitioner, then, must make up his mind to work.

It is not quite certain, however, whether all has yet been done which might be done in the elaboration of *fixed general principles* of pathology and therapeutics; or at least, whether the time has not now come for a more definite and direct attempt. It is not possible to remain indifferent to the vast and secure strides which medical science has made of late years. If some of our more recent systematic writers be compared with those popular a quarter of a century ago, and these latter be compared with the writers of the twenty-five years preceding them, it will be seen that medical knowledge has recently advanced in a very accelerated ratio. A more refined visual and chemical analysis has been so applied to physiology and medicine by the aid of the microscope and improved chemical science, as to constitute an entirely new instrument of research, and thereby obscure phenomena have been elucidated, and their hidden relations brought to light. Large general laws of nutrition and development are being propounded; the chemical and microscopic anatomy of the blood (if the phrase may be permitted) is being brought to bear on an improved system of physiology; morbid and morbific changes in that fluid are consulted more and more in relation with structural change and functional disorder; and above all, the foundations of the science of vital dynamics are being securely laid on a better knowledge of vital forces, from whence fixed principles in pathology and therapeutics will eventually arise. The medical profession may, most justly, congratulate itself upon the cheering prospects before it, and may reasonably hope that systematic writers will not be wanting to re-cast and combine the vast mass of materials which the labours of the past and present generation of medical inquirers have placed at their disposal. The time is favourable for the attempt. In the first place, the number of the self-styled "practical" men is rapidly decreasing and giving place to a class with a better knowledge of what is really and truly practical. This class more clearly perceives that the crudest theories are quite compatible with an arrogant disdain of the theoretical, and that, in fact, the assumption of the title of practical is only a stroke of policy to hide a profound ignorance of the literature and science of the profession. This largely increasing class is also aware that an assiduous cultivation of

the reasoning, as well as of the observing, faculties is necessary to the formation of the sound practitioner, and that a course of study, limited to manuals and medical reading-made-easier, can never develop the intellect or enlarge the mind; while it will almost inevitably lead to the empiricism of which the homœopathic sect offers the most perfect type. But perhaps there is nothing more satisfactory in the whole course of recent events in the medical world, than the good which has resulted and will result from the evil of licensed empiricism. With unrivalled sagacity, not a few have acted on the principle "Fas est et ab hoste doceri," and have turned the doings of various empirics to practical account. The influence of the nervous system, and of the mind through the nervous system, has been better recognised; the experience of Currie and others as to the simpler treatment of fevers and febrile diseases, has had the attention it merited; and the importance of dietetics and hygiène, in the practice of medicine, more clearly perceived and acknowledged. The scepticism which, on the one hand, despises medical art, and the polypharmacy which, on the other, humiliates it, have alike received a check. It may be now more than ever truly said, in the words of a popular writer of the last century—"Ratum enim apud omnes manet NATURAM esse optimam morborum medicatricem: et frequentiores affectus ipsi commissos, annuente SUPREMO VITÆ ET NECIS ARBITRIO, sponte sanari; in aliis verò haud exiguam opem conferre artis præsidia, dummodo dosî congruâ præcipiantur, et tempestivè, vel lentâ festinatione exhibeantur."*

This full recognition of the curative efforts of "nature," and the adoption of methods of cure which have for their object the assistance of those efforts, will hardly constitute an important step in advance, unless more accurate information be acquired as to the forces and the dynamics of the machinery by which these beneficial results are attained; or, in other words, unless an improved physiology and pathology supply *principles* for our guidance, so that we can discriminate readily between phenomena originating from the proper exercise of the powers of the organism, and those resulting from the direct operation of morbific agencies; for it is on a due discrimination between these two groups of phenomena that a rational and scientific treatment can be based. These curative efforts of "nature" have been recognised more or less in every age, and methods of treatment have been successful in proportion as they have been formed in reference to the *vis medicatrix;* but the adoption of such methods has been due rather to great instinctive sagacity, than to a full philosophical appreciation of the properties of organized matter. The inventors have closely followed nature, but just as a workman would follow instructions in the management of a machine, while still ignorant of its principles of construction; or they have copied nature with a knowledge of her general aim and ends, but ignorant of details. No more striking instance of this class of great physicians can be mentioned than Sydenham: it was to the intuitive sagacity with which he observed and appreciated the course of nature in disease, that his persistent fame is due. With admirable judgment and precision, he recognised and applied that great principle of perfect adaptation by a universally operating Providence, which is, and must

* Synopsis Universæ Praxeos Medicæ, &c. Nova Editio. Auctore Jos. LIEUTAND, &c. Parisiis 1770. Proœmium, p. xiii.

be ever, the only basis alike of psychology, physiology, and pathology; and with no less sagacity, he discriminated between the results of that principle in operation, and the results of morbific agents. Thus,—to mention one of many examples,—in his discourse on the Pestilential Fever (the great Plague of London), he observes, "From all this it clearly follows, that the physician who, in the cure of other diseases, is *bound carefully and closely to follow the path and conduct of Nature,* must here renounce her guidance;" and then goes on to argue for active treatment by bleeding and diaphoresis. In the same discourse, he fully defines the meaning he attaches to the term Nature, and with such instructive lucidity, that the passage merits quotation:—

"One concluding observation I have yet to add, and I do so, lest any one twist my meaning by an improper interpretation; or at any rate, lest he misunderstand it. In the foregoing discourse, I often use the word *Nature*, and attribute to it various effects; just as if I pictured to myself under this name something universally diffused throughout the whole framework of the world; something that ruled, as it were, and regulated, all substances reasonably and with intelligence; something, in short, like the *animus* [*anima*] *mundi* of some philosophers. Now, for my own part, I dislike innovation, both in fact and language; so that throughout my pages I have used the term *Nature* as an old word with a limited meaning, which I believe all understand, and some adopt. Hence, as often as I apply it, I mean the whole complication of natural causes—causes which, in themselves, are brute and irrational, but which, nevertheless, are regulated by the highest reason, and which, under its guidance, perform their functions and exhibit their operations. The Supreme Deity, by whose power all things are produced, and upon whose nod they depend, hath, in His infinite wisdom, so disposed all things, that they betake themselves to their appointed works after a certain order and method; they do nothing in vain; they execute only that which is most excellent, and that which is the best fitted for the universal fabric, and for their own proper natures. They are engines that are moved, not by any skill of their own, but by that of a higher artificer."*

Physical philosophy, as well as anatomy and physiology, have largely added to the proofs of the existence of this great force; but it could hardly be defined in clearer language. We now know that this principle of adaptation is as universal as the force of gravity, as constantly in operation, as fixed and definite in its results. To the physiologist it is a familiar fact,—so familiar as to lose much of its weight,—that the simplest microscopic organism displays the operation of this principle of beneficent adaptation as much as the most highly developed. To the astronomer it is equally as familiar a fact, that the same principle of beneficent design is in operation amongst the vast masses of the universe. By the calculus of probabilities, La Place has shown that the sun's rising again on the morrow of any given day is two million times less probable than that the motions in our planetary system are the result of perfect design, with a view to the *stability* of that system. Since the time of Sydenham, moreover, anatomists and physiologists have done much to elucidate the modes of action of this principle; in particular we now understand better than in his day, the pathology of that portion of the nervous system (and its appendages) which has recently been denominated "the true spinal system," and which is the seat and instrument of a large number of automatic actions, the end

* The Works of Thomas Sydenham, M.D. Sydenham Society's Edition, vol. i. p. 119. (Dr. R. G. Latham's Translation.)

and aim of which is the conservation of the individual or of the species. This law of adaptation to beneficent ends—the care and preservation of the individual—is so fully recognised on all hands as the guiding principle of the "reflex function," that no additional testimony need be adduced on this point. The extension of the same law of action to the encephalic portion of the nervous system is rapidly in progress; and the time is not far distant when it will be discovered to be the only sound basis for cerebral physiology, as well as of any new systems of psychology which will necessarily arise therefrom. The application of the same doctrine to all the various vital processes, which go on *independently of a nervous system*, as well as in connexion therewith, although necessary to progress, is not so probable; nevertheless, as the advantage to practical medicine of further researches in this direction becomes appreciated, they will be undertaken, and the *desiderata* for medical art of well-founded general principles be attained. Histology, in this respect, will add largely to our knowledge, for it will enable us to understand the more recondite operations of the great principle, as well in *vegetable* as in animal organisms; but whether or no, researches into these operations can no longer be limited to the nervous system, or even to animal tissues. This extension of the doctrine of "reflex function" will be a large stride in advance; for rich as have been the results to practical medicine of the application of comparative anatomy and physiology to pathology, much more will be gained by a diligent study of the processes of *vegetable* life, and of the forces by which they are carried on.

But, indeed, the time has already arrived for a wider and more philosophical generalization of vital phenomena in relation to medical art. The resuscitation, in the nineteenth century, of the leading doctrines of the HUMORAL PATHOLOGY current in the seventeenth, is not without deep meaning, nor of trifling importance. It not only indicates a more general recognition of the existence of that fundamental vital force upon which that humoral pathology was based, but it will also render the vast stores of experience and of learning, accumulated during the period when it was current, available to modern uses, inasmuch as the great truths they contain will now be readily sifted from the errors and crude theories with which those truths are mixed up. Looking at the mass of printed pages before us, we have felt that an abstract, or even a critical analysis, of their contents, was a hopeless attempt, if anything like justice was to be done either to author or reader; we therefore thought it better to sketch out some such wider and more philosophical generalization, taking as our guides the mediæval and more recent views; and as illustrations may be more useful than abstract considerations, we propose to consider the two large groups of diseases to which the doctrines of the humoral pathology are now being more particularly applied—*fever* and *gouty diseases* Dr. Wood has given a short history of the theories of fever, and we quote his statement of the doctrine of the humoral pathology as an introduction.

"Not only in the case of fever, but of all other diseases, it was supposed that the blood becomes contaminated or depraved, either by the reception of noxious substances from without, or in consequence of changes going on within the body; that the system is excited by the presence of these impurities, either to direct efforts for their expulsion, or to a course of elaborate action, so as to fit them for expulsion; and that, in the language

of Sydenham, a disease is no more than a vigorous effort of nature to throw off the morbific matter, and thus recover the patient. This is the sum and substance of the humoral pathology."

Grafted upon this general doctrine, and flowing out of it, were various theoretical notions of fermentation, concoction, &c., which lead to injurious and unnatural methods of treatment. The more sagacious of the school repudiated these notions. Thus Sydenham remarks: "I cannot bring my brain to comprehend the meaning of those physicians that are continually talking about the administration of remedies that promote the concoction of the febrile matter—points which they insist upon when called in at the beginning of the disease. Why! the fever itself is Nature's instrument. . . . . *The concoction of the matter of fever simply means the separation of the sound from the unsound.*"*

Sydenham expressly repudiated the use of the terms *fermentation* and *ebullition*, when treating of continued fever, preferring the term *commotion*. "I begin with observing," he remarks, "that the inordinate commotion of the volume of the blood, whether cause or concomitant of this fever, is excited by nature in order that either some heterogeneous matter, incorporated with our humours, and inimical to them, may be eliminated, or else that the blood itself may be transmuted into a better diathesis."†

Dr. Wood appears to think that it would be quite supererogatory to attempt to refute doctrines of this kind. The "fanciful notion of a vis medicatrix naturæ," to use his words, has no part in his pathology; and as to the more recent humoral views, while he admits that the blood is often greatly altered in the bilious, remittent, and yellow fevers, as well as in others, and that it is possible, in some instances, that this alteration is primary, and the direct cause of the other changes which occur, he insists that, "in the present state of our knowledge on this point, the assumption that all fevers, or even the particular fevers mentioned, are dependent exclusively on the morbid state of the blood, is quite unwarrantable." Upon examining more in detail the views of Dr. Wood, as to the nature of fever, we find that the word "exclusively" largely qualifies his meaning, and that there is much in which we agree with him, in common, we think, with the majority of practitioners in this country. Dr. Wood considers fever to be a disease of the whole system; that it is sometimes essential and idiopathic, sometimes symptomatic; that when symptomatic, the cause is local, as when there is local inflammation. As to the idiopathic group, and as to the general disturbance which occurs in the symptomatic as well as in the idiopathic fevers, Dr. Wood is not able to state anything distinctly, and frankly confesses his inability. He remarks: "Together with disorder of the solids, there is very frequently an altered state of the blood; but the question has not yet been settled whether this is an essential part of the disease, and important in the chain of causation, or a mere incidental effect. We are thus in a condition of uncertainty upon almost all points." We need hardly remark, that general principles like these are of little practical value. They neither supply rules of art, nor explain the curative operations of nature, which all experience proves to be the best guide. In our judgment such remarks hardly do justice to existing medical science. Wunderlich is much more definite and explicit. Recognising the constitutional character of fevers, he notes that there is a

* Op. cit. vol. i. p. 54. † Ib. p. 44.

certain habit of the functions generally—a normal condition—and that deviations from this condition are disease as important as those dependent upon structural changes. An abnormal susceptibility of the functions in relation to external impressions and local disorders constitutes constitutional irritability. Increased excitability, and rapidity and disorder of the functions, not momentary, nor quickly passing away, is *constitutional irritation*, or FEVER. Retarded, imperfect, or paralyzed functional action, is torpor. The relations of these three forms are numerous. As to causation, constitutional irritability, or torpor, is an habitual condition. It is a defect dependent upon hereditary conformation, age, diseases previously experienced, modes of life, and the like. It may have its origin in the nervous or nutritive system, &c. Under given circumstances, this condition may be developed into constitutional irritation, or fever. 1. There may be a predisposition to this form of disorder, from causes specified. 2. Changes in the external relations of the organism, as of temperature, barometric and hygrometric conditions, the winds, climate, and the like. 3. Every deviation from the normal condition in the blood will induce fever, or constitutional irritation, provided it be immediate and sufficiently extensive. All morbid conditions of the blood predispose to disturbance of function and febrile action, but most certainly of all, any considerable increase in the fibrin; febrile phenomena are manifested in the highest degree, and with early symptoms of adynamia and paralysis, *when the fibrin is diminished in quantity, when foreign and poisonous substances enter into the circulation, or when abnormal transforming processes are set up in the blood.* 4. Violent muscular actions, violent exertions, excessive sexual indulgence, more or less develop constitutional irritation and febrile phenomena of the adynamic type. 5. Painful affections, sensorial irritability, the occurrence of secretions for the first time, as the menstrual and lacteal, or the sudden suppression of such secretions, strongly predispose to, and sometimes induce, febrile phenomena. 6. Local disorders, classed by Wunderlich under six heads,—namely, whether acute or chronic; as to the tissue affected; as to the extension of the chronic disease; as to the sympathetic irritation; as to the changes in the blood which it induces; and as to the kind of organ which is the seat of the local disease. Like Dr. Wood, Professor Wunderlich thinks it not possible, in many cases, to determine whether fever is idiopathic or dependent on local causes, nor whether the local changes are primary or secondary, inasmuch as we are so much in the dark as to the origin of the phenomena, or their causal connexions. Like Dr. Wood, Professor Wunderlich takes no note of the great basis of all vital dynamics, that all-pervading "something" of Sydenham, in virtue of which "all things betake themselves to their appointed works after a certain order and method," and in virtue of which, therefore, the phenomena of disease are subject to that same determinate order and method. It is in thus ignoring this great and most fundamental principle of vital dynamics that we think the authors before us are defective. The "fanciful notion" of a vis medicatrix is so far from being fanciful, that the reparation of injury to the organism is as much a part of its nature and mode of existence, as its development from the primary cell through all the phases and grades of its existence. It is not possible to institute any *fundamental* difference between the vital force which produces a limb

for the good of the animal, according to the normal and fixed order of development, and that force which reproduces a limb equally according to a normal and fixed order, and equally with the same object when the limb has, by accident, been severed. To our minds, the process of nutrition by which growth is maintained is not *essentially* different from that process of nutrition by which waste is repaired; neither can we think that if the great principle of adaptation to beneficent ends be considered as the basis of vital dynamics, it is logical to maintain that after providing in every way for defence against or for the removal of noxious agencies generally, it stops short at the removal of those which cause special forms of injurious action. The minor proposition is surely included in the major.

In adopting more particularly the humoral pathology of Sydenham, and of the great thinkers of the seventeenth and eighteenth centuries, it is by no means necessary to adopt their errors or to neglect the truths of modern pathology. The blood is undoubtedly the primary constituent of the body, as the capillaries are the primary mechanism. It is upon the histological change in the blood and in the tissues that all vital phenomena essentially depend. The heart and vascular system at large are but an hydraulical machinery for its due distribution: the pulmonary system is its aërating apparatus; the gastro-intestinal supplies the solid and fluid materials; the nervous systems, both voluntary and involuntary, are but special instruments constituted to put in motion, co-ordinate, and harmonize, all this varied machinery of the organism, and to defend it and them, so that the great object of the universally innate principle of Providence—the well-being of the created thing—may be fully attained. When this great principle works in a simple cell, it works with as admirable adaptation in all these respects as in the most complex of organisms; but inasmuch as in it there are no machines to co-ordinate other than the cell-wall and the granules, it provides no nervous system; as the circulating fluid is limited to the cell-cavity, aëration and nutrition are direct, and there is therefore no need of stomach, heart, or lungs; equally without distinct excreting organs, the circulating fluid is still duly depurated; and if it be not depurated, or poisons be introduced, the cell dies. Histology, while it establishes the great fact that the law of more complex development is simply a continual specialization of function, shows to us also that the organic elements essential to vital action are the containing tissue and the contained fluid; the former constituting the analogue of the apparatus whereby nutrition, aëration, and depuration, are perfected, the latter the analogue of the blood, and fulfilling its functions. From this point of view it is seen that the blood is the primary and principal seat of vital action; that for the maintenance of its purity all the varied processes of animal life are instituted; and that without that purity, they must all languish.

The great pervading principle of adaptation recognises to the full extent this fundamental and primary importance of the blood in the economy. With more complex development, every imaginable precaution is taken for the supply of material to it, perfectly elaborated and fit for the functions which the blood has assigned to it. Machines for procuring, seizing, tearing, and lacerating the raw materials of the most diversified design and most ingenious construction are developed; apparatus expressly fitted to prevent the entrance of improper materials is provided, manifesting

not less ingenuity and perfection of adaptation; if unfit materials by chance escape these arrangements, a further provision is made for their expulsion from the recipient viscus, and should they escape these inner guards of the system, and make good their entrance into the citadel of life, the depurating organs come into active operation and effect their expulsion—with what rapidity and success, under ordinary circumstances, experiments in toxicology and therapeutics abundantly demonstrate. Not less admirable prevision and masterly skill preside over the due aëration of the blood, and so important, in short, in the scheme of LIFE, is the regular supply of solid, liquid, and gaseous elements, to the circulating fluid, that by means of the co-ordinating apparatus—the nervous system—the whole powers of the individual are concentrated, if the supply be defective, upon its attainment. Thus it is that hunger, thirst, and the desire for air, are the most urgent and most uncontrollable of the instincts. But it is of great importance to the pathologist, to notice the fundamental fact, that it is the morbid condition of the blood which excites the whole organism to desperate efforts, and not local and minor changes in the organs themselves. The dry throat is not the sole cause of the sensation of thirst and the concomitant agencies; it is only a secondary result of a diminished supply of fluid to the blood, in virtue of which its depuration is prevented, and the salts and used-up materials that ought to have passed off by kidneys, skin, lungs, and intestinal mucous surface, are retained, because the solvent is no longer present. Hunger is not, in its full intensity, caused by an empty stomach, but by that condition of the blood which a check in the supply of nutrient material causes. Hence in *tabes mesenterica*, more or less hunger is felt, however full the stomach may be. Great facts like these throw a flood of light upon the relations of all those diseases in which exalted functional activity of the nervous system is conjoined with blood-disease; the latter cannot possibly occur in any degree and not the former result in the same proportion, simply in virtue of the conservative efforts of that great instinctive principle we have referred to.

It would, however, be a most fatal error, if the attention of the pathologist were limited to what may be termed the phenomena of normal re-action. The due performance of the functions of all these diversified machines is certainly dependent upon a healthy condition of the circulating fluid; if this be wanting, the heart and vascular system labour from abnormal stimulus; the innervation of the nutrient, depurant, and aërant apparatus is imperfect, from imperfect action of the co-ordinating apparatus, and thus general functional disorder supervenes, constituting true disease, and giving rise to further morbid complications and re-actionary changes. To discriminate correctly between the phenomena of normal reaction and the phenomena of diseased reaction, is of the highest importance in practice, and the necessity of such discrimination has been recognised by the leading minds of all ages. There is indeed no more profound principle in therapeutics, nor a principle upon which more refined observation and a sounder physiology will throw greater light.

In applying this doctrine to fevers, we see at once that just as there is healthy and unhealthy inflammation, so there is healthy and unhealthy fever, paradoxical as the use of the terms may appear. What, then, are the

phenomena of healthy fever, or rather of that normal reaction of the organism against poisonous agents moving along in the current of the blood, in those fevers in which it is fully allowed that the presence of a febrific poison is the cause? Obviously, the symptomatology of fevers looked at from this point of view is nothing more than a branch of toxicology; indeed, pyretology might be termed pathological toxicology. Now we know that it is through the depurating organs that a poison is carried out from the blood, and that, consequently, any increased action of these organs occurring in the course of blood-fevers, may be considered as a part of the normal reaction, or of the healthy phenomena of fever. To this group, then, we can certainly refer the diaphoresis, diuresis, and diarrhœa, occurring in the course of blood-fevers; probably also increased pulmonary transpiration (in cases in which the respiration is hurried), and increased salivary and biliary discharges. We may also refer to the same group two other leading phenomena of fever—namely, increased vascular action and thirst; the latter developed to acquire an increased supply of diluent fluid; the former, to transmit the blood more rapidly through the depurant organs. Whether the increased heat be a part of the healthy or the morbid phenomena is not so clear. It is usually attributed to the increased rapidity of the circulation, and the consequent increased oxidation of the tissues; in so far as it is indicative of excessive waste, it is morbid; but it is not altogether improbable that the increase of temperature is a part of the curative process, for this may demand for its more perfect performance a higher degree of heat in the blood and tissues. Be this as it may, if in the course of a blood-fever these symptoms disappear—that is to say, if the pulse becomes feeble and slow, the thirst ceases, the body cools, and the secretions are arrested, the healthy phenomena have become unhealthy.

Toxicology teaches us that poisons have a *local action* upon tissues and organs as well as a general action through the blood. This local action differs in degree according as circumstances differ. To take mercury as an illustration. If this poisonous metal be administered therapeutically, even in very small and carefully-watched doses, in cases of nephria, in which the excreting tissue of the kidneys is destroyed, it is difficult to prevent dangerous ulceration of the mouth and fauces supervening. This appears to depend upon two distinct causes; firstly, the drug is not carried off from the depurating organ so rapidly as under ordinary circumstances; and secondly, the pre-existent morbid condition of the blood facilitates adynamic inflammatory action. So in fever-poisoning, the extent, at least, of local action is determined by the previous condition of the blood, and thus it is that local adynamic inflammation, indicated by sloughing, gangrene, &c., is apt to occur in all fevers, in which there is a morbid impoverished condition of the blood at the time when the poison is received into it, such as occurs in scorbutus, in the malarious, syphilitic, and uræmic cachexiæ, and the like, or in all in which the blood is rapidly devitalized. These phenomena characterize the *malignant*, *typhoid*, and *adynamic* forms of febrile diseases. Again: all things being the same, the *amount of poison* admitted determines the degree of both general and local action, so that a large dose is much more influential than a smaller. This has an important bearing on the history, etiology, and semeiology of febrile phenomena. Throughout the whole group of malarious fevers, to

mention an illustration, we have a very great variation of the phenomena, induced simply by this difference in the amount of dose of poison taken into the blood. It is evident, indeed, that just as with drugs generally, the reaction against febrific poisons, and the morbid and morbific effects they induce, depend upon *several* circumstances other than the dose, those concerning the *individual* being, however, the more important to remember. Precisely as in therapeutics, the effects of the same dose of opium given to several individuals will be very different, so it is with febrific poisons. Thirdly, the circumstances which determine action in any *particular* tissue or organ, are very various, as well as the susceptibility to morbid action in the different organs. Thus the fact, whether the poison is solid, fluid, or gaseous, is important; so, also, the great fact that organs, often under circumstances of this kind, put on a depurant vicarious action, whereby poisons differing in their nature, and exciting a *different* series of reactionary phenomena under ordinary circumstances, may, under these circumstances, excite *similar* phenomena. But whatever may be the exceptional conditions, it may be stated, generally, that a local inflammation occurring in the course of a blood-fever, is the direct *result* of the blood-poisoning, and does not belong to the group of the normal or healthy reactionary phenomena of fever. We can therefore say, that the cutaneous inflammations occurring in the large group of exanthematous fevers, are not essential to the depuration of the blood, but are morbid phenomena in the exact sense of the word. The old humoral pathologists adopted a contrary view, and upon that view founded the hot and stimulant method of treating variola and other fevers of that group. Modern therapeutics have wholly discarded the method of treatment, although, under the term *zymosis*, the theory is still entertained more or less. These eruptions, therefore, as well as all the inflammations of mucous surfaces, such as the buccal, pharyngeal, pulmonary, gastric, and intestinal, which occur in the course of fevers, must be considered as phenomena, not essentially resulting from the curative instinct, but morbid effects of the febrific agent, and themselves giving rise to secondary series of instinctive reaction and other febrile phenomena, or, in other words, to inflammatory fever. It would certainly be a mistake, we think, to take even these latter out of the general category. The febrile reaction set up in the reparative process, after injuries or local derangement, is really a part of the general movement towards health; and it appears to be by no means improbable, that the phenomena thereof are dependent upon changes in the nutritive constituents of the blood, but especially (as is now more generally thought, and stated by Wunderlich) upon an increase in the fibrine. Hence, inflammatory fever not only takes its place among blood-diseases, but its phenomena may be divided into the purely reactionary, or healthy, and the purely morbid. A more careful analysis of the phenomena of inflammation and of inflammatory fever in relation to what have been usually termed idiopathic fevers, will lead to highly beneficial results. It is a conclusion not less flowing from theory than from observation, that the secondary results, or complications of fevers, are much more important because much more morbid than the fever itself, except when the co-ordinatory apparatus—the nervous system —is directly involved. The experience of the observant practitioner is clear as to the greater fatality of the fevers with inflammatory sequelæ or

complications, and as to the greater danger of the local than the general disorder. Hence the importance of a more perfect natural history of fevers, so that these inflammatory complications may be prevented. Inasmuch as prophylaxis is all-important in treating them successfully, it is to be regretted that this great practical point has been so much lost sight of, or misapprehended, in discussing the question as to whether fever is a general disease, or dependent only upon local inflammation or irritation. A philosophical analysis of accurately observed phenomena will do much, both towards sound theory and sound practice in this respect.

To secure, then, the best results, it will probably be found necessary to consider febrile pathology as a branch of toxicology. This step would have one most excellent effect—namely, to determine more accurately the *etiology of fevers*, and the *modus operandi* of febrific poisons, by principles drawn from the general facts of toxicology. It is truly lamentable to see how much misunderstanding might be avoided by larger views of this kind, as well in regard to the general pathology of fevers, as to the doctrines of contagion, and the generation *de novo* and reproduction of the *materies morbi*. According to the present method of discussing them, the whole group of the *chronic blood-diseases*, induced by febrific poisons, and so predisponent to the acute forms, is either lost sight of altogether, or occupies but a subordinate position. Yet nothing is more certain, we think, than that the chronic blood-diseases of this kind, whether considered as distinct affections or as predisposing causes, are equally important as the acute. The chronic action of malaria upon the nervous system, although so admirably demonstrated by Macculloch, has by no means had that attention given to it which this widely-spread and very insidious form of disease requires, while the chronic influence of animal and fæcal effluvia, and of retained *excreta*, is hardly recognised at all. In treating of the humoral pathology as applicable to gout, we will endeavour to illustrate this point. Whether we consider the acute or the chronic groups, it is obvious that as a branch of toxicology we must determine the etiology; or, in other words, ascertain the specific fever-poisons, and the phenomena they excite.

As to the etiology of fevers, we are much indebted to our American brethren. The vast extent of territory in the new world occupied by the English race comprises almost every climate; and although it be not so varied or extensive as the British empire, the fevers to which the inhabitants are liable are felt *at home*. To the American physician, whether of the States or Canada, the entire and extensive group of *malarious fevers*—comprising diseases of every degree of intensity, from simple ague to the most pernicious remittent, in which the nervous system is primarily affected, and which are characterized by phenomena not unlike those of Asiatic cholera —is presented, under circumstances of great interest, while of late years the vast crowds of emigrants from Europe have introduced and spread widely the *ochletic* class. We are inclined to think that one of the best nosological arrangements on record of febrile and contagious diseases is due to Dr. Hosack, and his commentator Dr. Smith, in whose work (1825) on the 'Etiology and Philosophy of Epidemics,' it may be found. The arrangement of Wunderlich is certainly much less clear and suggestive. In the former arrangement there are five genera. The first comprises diseases communicable by contact only, and constitutes a very natural group, if scabies

hydrophobia, and vaccinia, be omitted, inasmuch as it includes syphilis, sibbens, yaws, the laadda of Africa, and elephantiasis; to which, possibly, radesyge and pellagra might be added. The second comprises the acknowledged exanthemata, influenza, pertussis. These are communicable as well through the atmosphere as by contact, and generally attack a person but once. To this genus some recent researches enable us to add, exanthematous typhus, probably Asiatic cholera, one form of yellow fever, plague, the 'relapsing' fever, and dengue. The third genus includes fevers caused by the exhalations from the sewers and filth of cities, and the soil of marshes, &c. These fevers are the intermittent and remittent fevers, and those characterized by yellowness and black vomit. Modern researches would suggest a division of this into two groups, the one comprising the purely malarious, induced by vegetable effluvia, and the other, those induced by animal effluvia, especially civic filth—the true miasmatic. The fourth includes jail, hospital, and ship fevers; these result from the effluvia given off by the pulmonary, cutaneous, and other excreta of the human body, accumulated in small and unventilated places, and acted upon by heat. This group should be limited to those fevers caused by *ochletic* miasm only, to the exclusion of the fæcal, included in the previous genus. The fifth is the mixed class of fevers produced by the union of the two preceding febrific poisons.

It will not be unprofitable to show how this arrangement, modified on the principles of a reformed humoral pathology, may be made the basis of an entirely new nosology of fevers, which shall provide for, and explain, many of the apparent anomalies so puzzling to the student and practitioner. The first genus is a group which might be left wholly undisturbed, with the exception stated. Hydrophobia and vaccinia might be transferred to a new genus, to include those fevers communicated from brutes to man—the epizootic. The second genus might also remain, with the additions suggested; but the three remaining genera might be broken up etiologically, and the species re-distributed. We would, in the first place, allot a genus to those arising exclusively, or principally, from the effluvia of decaying *vegetables*, or their *excreta*, and term it the *malarious*. Their phenomena are characterized by periodicity, and the action of the poison is manifested on the nervous system generally, and on the stomach, liver, and spleen locally. The *chronic* diseases it induces are splenic, gastric, and hepatic congestion and inflammation, malarious chlorosis, spanæmic cachexiæ, local neuralgiæ, centric diseases of the nervous system, of an adynamic type. The *acute* diseases this poison induces, are the great family of intermittent and remittent fevers. We would group another class of fevers, manifesting as a leading symptom gastro-enteric irritation, under the term *fæcal*, or true *miasmatic*. In this group the poison derived from the decomposition of the intestinal excreta of man and animals, acts upon the depurating surface from which it is normally excreted, and excites the non-contagious forms of gastro-enteritis, gastric fever, dysentery, diarrhœa, and summer cholera. In these the local irritation is due, in the first instance, to the operation of the instinctive curative process of elimination, so that the diarrhœa and gastro-intestinal effusion are depurative. When conjoined with local inflammation, ulceration, and perforation, it is to be considered as a morbid, and not a curative symptom.

The pulmonary, pharyngeal, and cutaneous surfaces are extensive excreting surfaces, and want of personal cleanliness (that is to say, the retention upon its surface of the matter excreted from the skin), and imperfect ventilation, with overcrowding (that is to say, the retention of the pulmonary and tonsillar excreta in the air, and their constantly repeated respiration in large quantity), induce a group of fevers which may be termed *ochletic*—a term first applied by Dr. Gregory, and well adapted to designate the group. It comprises those forms of non-contagious typhus and typhoid, in which pharynx and tonsils suffer, the lungs manifest a low form of pneumonia or bronchitis, and there is more or less inflammatory change in the skin, shown either as rose-coloured eruptions, petechiæ, vibices, or as a dusky redness. The phenomena of neither of these two groups exhibit periodicity in the same way as those of the malarious, and, like the latter, they may recur again and again in the same individual, provided he be the recipient of a sufficiently large dose of the poison. It is, moreover, a characteristic common to all, that the true fever paroxysm, or entire series of paroxysms, has a definite duration; modified, however, very variously in this respect, by the secondary fever, and by the character, seat, and extent of the local complications. A general principle of toxicology is also common to all—namely, that the constant presence of the poison in the blood renders the system much more tolerant of large doses; a condition which, in reference to the poisons of these groups, is termed *acclimatization*. Whether there be provision made for a more regular or more efficient elimination of the poison, or whether it is that that great instinctive principle of adaptation to external agencies, which causes so large a number of changes in the external form and the internal mechanism of organisms, comes into operation under these circumstances, is doubtful; but the fact is certain. Just as persons become inured to large doses of poisonous drugs, as opium, tobacco, alcohol, &c., so the born dwellers in marshy districts suffer less than immigrants, and the seasoned denizen of the town than the stranger from the country.

The febrific poisons of these groups are gaseous; but there is another group holding a somewhat indeterminate position—namely, that comprising fevers in which putrescent animal (and perhaps vegetable) matter is introduced into the blood, either in the fluid state *directly* through a wound, or *indirectly* in a gaseous state, by absorption through the lungs or skin. It is probable that there are two divisions in this group—namely, those derived from this source when the fluids of the putrescent organism contain a specific fever-poison (as in death from variola or erysipelas), and those in which there is putrefaction simply. Distinct from all, however, is the great group, in which, with tolerably well-marked periodicity, definite *inflammation of the skin* or *mucous surfaces* is a leading phenomenon: in which there is a something generated in the blood, such that when introduced, under suitable circumstances, into the blood of a healthy person, it induces a similar fever; and which, having attacked an individual once, seems to be incapable of inducing again in him during his lifetime the same kind of diseased action, or at least in the same degree. These fevers, from their local complication, have been termed *exanthemata;* but since it often happens that the local inflammation is so slight as to be almost invisible, or being limited to the inner surfaces is never seen, it is doubtful whether the group would not be better designated *non-recurrent;*

and the local complication considered as a secondary and corroborative characteristic, rather than a primary. The cutaneous inflammation observed in plague, miliary, gastro-enteric, relapsing, and exanthematous typhus, in one form of yellow fever, in Asiatic cholera (Indian typhus), in influenza, dengue, and occasionally in pertussis, taken with the fact of the alleged non-recurrence and contagiousness of these epidemical fevers, would warrant us in adding them to this group. We should thus have three principal groups, the specific, or non-recurrent fevers; the miasmatic, or typhoid; and the malarious, or periodic.

Dr. Wood will probably have no difficulty in conceding to us the *general* correctness of this arrangement; the principal fevers of the groups lie, however, on debatable ground, and it has been found difficult to assign to typhus, plague, yellow fever, Asiatic cholera, &c., their exact nosological position. This difficulty is, we think, to be found in the great fact, that where men congregate in masses, no one poison is found in the blood alone at the same time, or, in other words, that in the great majority of fevers, and in all of the epidemic class, there are two or more poisons in the blood, and that, consequently, the febrile phenomena are of a complex character. Thus malaria may be combined with sewer or fæcal poison; the latter with ochlesis; or the whole three with a specific poison, as variola. The combination of malaria and fæcal miasm is very common, and gives rise to all those forms of intermittents and remittents which are characterized by the supervention of abdominal symptoms, as dysentery, diarrhœa, ulceration of the intestinal mucous surfaces. The combination of fæcal miasm with the poison of the exanthemata is also no uncommon occurrence, and is marked by the same complicating symptoms of gastro-intestinal irritation or ulceration. Hence it is that the etiology of fever is an important element of diagnosis. Without a clear perception of the nature and origin of the febrific poisons, and of their *modus operandi*, accurate diagnosis is in fact impossible, except in cases in which the characteristic phenomena are unmistakably manifested, as in regular intermittents, bubo plague, or examples of the exanthemata with a distinct eruption on the skin. Hence also it is, that no practitioner will overlook the prevailing type of fever, or, in other words, what specific poison is in operation, while he also bears well in mind the important circumstances of climate, temperature, and public and personal hygiène as pointing out the source and nature of the complicating febrific poisons.

It is not altogether impossible to classify the phenomena arising out of these complex combinations. Malaria, when acting independently of local inflammation, is purely intermittent in its influence; in proportion as local inflammation is excited, the paroxysmal character is lost, and the fever is remittent or continued; consequently specific poisons exciting local inflammation mask the true malarious phenomena. These are, however, also characterized by a *depressing influence* on the co-ordinating apparatus—the nervous system—and on the vitality of the blood. Hence in all cases of complex specific fever in which there is a large malarious element, there are highly-developed adynamic phenomena. To this group of complex specific fevers may be referred the epidemic cholera (Indian typhus) of Asia, the yellow fever of the Western Continent, the Levant plague, and those epidemics which ravaged Europe in the middle ages, previously to the advance-

ment of public hygiène, and which were sometimes influenza, sometimes exanthematic typhus, bubo typhus, scarlatina, and the like. The *fæcal miasm* predominating, develops intense *abdominal* symptoms; complex fevers of this group appear, therefore, in camps, and in all large bodies of men where no provision is made for the removal of fæces, in cities of temperate climates during the heats of summer, and especially after a period without rain, when a large accumulation of fæces in the sewers is acted upon by a high temperature. In these instances, diarrhœa, summer cholera, and dysentery, complicate the fevers arising from a specific poison, or usher in epidemics of abdominal typhus, or of continued fever with abdominal symptoms. When *pulmonary* and *cutaneous ochlesis* predominates, as it will in all races, nations, and individuals, that neglect due ventilation and cleansing of the surface, the specific fevers, accompanied with pulmonary and cutaneous inflammation, assume a malignant type. Hence, in some degree, the fatality of Irish typhus; hence, also, the destructive ravages of the exanthemata, especially scarlatina, rubeola, and variola, amongst semi-civilized and wandering tribes. In these it is probably the ochletic miasm which gives the adynamic character and destructive extension to the local inflammation. When *pulmonary ochlesis* especially prevails, the specific febrile poisons which specially act upon the aërating surface, are more prevalent and malignant; and as it is in cold countries that the pulmonary system attains its highest development, and during the cold seasons of temperate climes that the necessity for the exclusion of the atmosphere from dwellings arises, so it is in these countries and seasons that rubeola, pertussis, influenza, and the pulmonary complications of the exanthemata especially, and of fever generally, are most developed, and lead to the greatest mortality.

The fevers of *epizootic* origin have received so little attention, and their course and nature are so little determined, that it would not be possible to analyse them without entering into numerous details, for which we have now no space. It may be stated, however, that the whole question is closely connected with the mode of origin of the *specific* poisons, and with the question whether they originate *de novo*, and if originating *de novo*, whether they commence in brutes or in man. We are inclined to think that the more typical forms, as variola and scarlatina, may, in the first instance, have been epizootic, but that all the forms of fevers into which the miasmatic element enters, may originate *de novo* either in man or brute. When we consider the curious transformations and phases of life in the monads, in entozoa, and epiphytes, in cancerous growths, and in histological morbid changes in the tissues, it appears by no means improbable that the presence of the ochletic and miasmatic poisons in living blood, already predisposed to abnormal transformation by defective nutrition, aëration, or depuration, may induce such transformations in the elementary germs, that specific forms of cell-life may be developed from them within the circulating fluid, and pass off through those great depurating channels to which the atmosphere has access, or from which the excreta are eliminated; and that impinging upon like surfaces and entering a circulating fluid equally as favourable to parasitic life as that in which they were formed, they may be reproduced therein. The singular, and indeed almost incredible, minuteness of the germs of known *fungi*, as well as the natural history of the latter, favours the hypothesis, which has been broached by several observers lately, as to the *fungous* character of the

malarious, and specific poisons. If, then, from exposure to the conjoined operation of malaria, fæcal miasm, and ochlesis, new febrific poisons can be developed in the blood of man, we may conclude the doctrine is applicable to brutes, and fairly infer that epizootics may thus arise, the special cause of which, when communicated to man, may induce specific febrile diseases in him not necessarily similar in their pathognomonic phenomena, but transmissible from man to man as well as from brute to man. We need hardly mention glanders and hydrophobia as instances of communicable epizootics; we have seen cases of *murrain* in man, in which the communication could be as clearly traced as it is possible to be—namely, in farriers, who had to introduce their hand and arm into the mouth and throat of diseased cattle, in treating them medically. The *carbuncular* fever is also transmissible; and it is not at all improbable that the influenza itself is only a modification of the phenomena of a pulmonary ochletic fever constituting the epizootic pleuro-pneumonia, or lung-disease of horses and cattle. The relations of epidemic pharyngeal inflammations to the latter and to erysipelas are of importance. These questions, as well as how far the *sheep*-pox is the parent of some of the varioloid fevers, remain to be determined by a well-conducted series of observations and experiments.

Having thus applied the humoral pathology to the elucidation of the natural history and pathology of fevers, it remains to apply it to treatment. It is obvious that there are two great principles to be had in view—namely, the removal of the poison, or its destruction by antidote, and the treatment of its sequelæ—the same principles, in fact, as guide us in poisoning by arsenic or other poisons, mineral and vegetable. We can only mention a few illustrations of the mode in which these principles of treatment have been, or may be, carried out. Inasmuch as the primary phenomena of fever must be looked upon as an effort of the curative instinct to remove the poison, any remedial means expressly adapted to this end must be used with careful reference to these curative efforts. In this respect the *principles* of treatment laid down by the older humoral pathologists have great value; nor is their recorded experience of the results to be slighted, for they were far from indulging in that helpless system of looking-on, which practically leaves the patient to the worse than unaided efforts of nature, and so destroys all the value of medical art. Since, however, our knowledge of the intimate working of the mechanism is very imperfect, we need not, except in certain instances, do more than watch, and carefully attend to, nature's indications. Thus if there be a great desire for any particular thing, that desire is to be indulged. If there be urgent thirst, water or simple diluents are supplied abundantly; if there be a clearly expressed desire for salt food or stimulants, they are not withheld. If the skin be hot, it may be cooled by cold affusion, ablution, or free aëration, and the pure cool atmosphere may be freely admitted to the lungs. If there be total anorexia, food is not forced upon the patient; if there be any free excretion, as a diarrhœa, or diaphoresis, it is not unduly checked, but rather encouraged. In the treatment of 'continued fever,' Sydenham adopted a perfectly philosophical method little different from the modern. Noting the "concoction" of the blood which constitutes fever, he observed, "we must keep it within its limits, so that it coincides with the intentions of nature. If, on the one hand, it rage too much,

perilous symptoms will set in. If, on the other, it slacken into insufficiency, either the expulsion of the morbific matter will be hindered, or the efforts of the blood to reduce itself to a better state will be weakened."* He prescribed bleeding or omitted it, in accordance with these views. In persons of imperfect health, or with blood "of a weak character," as children, invalid youths, or men in the decline of life, "I keep my fingers," he says, "from the lancet. If I order venesection, the blood, weak enough even without being diminished, would be rendered incompetent to the work of despumation." In those "of an athletic habit and sanguine temperament," he says, "venesection is my leading remedy." And he adopts it with exclusive reference to the sequelæ—the local inflammations. "Neglect it, and you run the risk of frenzies, pleurisies, and such like inflammations." His next step is equally based on philosophical principles. It is so instructive that we will quote at length.

"After the bleeding, presuming it to have been necessary, I anxiously and carefully inquire whether the patient, at the onset of the fever, has been distressed by either vomiting or ineffectual efforts to vomit. If he have, I prescribe an emetic at once, unless, indeed, extreme youth or evident debility counter-indicate. Very necessary is an emetic, where there has been this previous tendency to vomit; since, unless the offending matter be cleared away, it will serve as a sink for all sorts of mischief. These will embarrass the physician during the whole of his attendance, and add greatly to the dangers of the patient. Of these, the most important, as well as the most usual, is diarrhœa. . . . . . As Nature partially corrects the malignant humours in the stomach, she passes them on to the intestines, and these become corroded by the continuous flow of acrid humours which the belly serves as source to. Diarrhœa *must* follow. . . . . Now the danger of the diarrhœa lies in this. The patient, weakened by the disease, grows weaker still."†

Sydenham here closely followed the indications of nature, for after "eyeing curiously the matters brought up by the emetic, and finding them neither remarkable for their quantity nor notably bad in their quality," he often wondered that the patient was "so much and so suddenly relieved." He administered the emetic by preference, "at the very beginning," when the vomiting, or desire to vomit, is manifested, giving late in the evening an anodyne draught. As to all these points, Sydenham's practice did not differ from that recommended, under similar circumstances, by Dr. Wood, in the treatise before us. Sydenham protested against attempting to check the vomiting in cholera morbus for the same reasons that he administered an emetic at the onset of continued fever. If successfully attempted, "fevers of a malignant disposition" supervene. "Such skill," he says, "is mischievous; stop the vomiting, and you replace it with a worse series of symptoms." He nevertheless strongly reprobated meddlesome medication. "The concoction of the matter of fever simply means the separation of the sound from the unsound. To accelerate this, we are not to meddle with attemperants, and I know not what . . . . . your evacuants and refrigerants are but hindrances; they procrastinate recoveries; and when health is coming of its own accord, they frighten it away. This I have seen often." The poorer patients got off better than the richer, for "as their purses would ill bear to be drawn upon for a long charge of physic," after the preliminary emetic and bleeding (if required), he enjoined them to keep

* Op. cit., vol. i. p. 46. † Loc. cit., p. 47.

their beds closely, and "drink oatmeal-gruel, barley-broth, or something of that sort;" to quench their thirst they were allowed "a little small beer with the chill off," and every other day took an enema of milk-and-sugar. "In this manner," Sydenham adds, "without any further complication of practice, beyond the mere addition of a purge towards the conclusion of the complaint, I sent them out of hand safe and sound."*

In treating the complications we find the same regard to principles. The bronchitic cough Sydenham treated simply by oil of almonds, "for cough medicines would only overload the stomach, which is already weak enough." After prescribing the best means of arresting *hæmorrhages,* he adds, "Now this must not be taken as if I recommended every hæmorrhage to be cured in this off-hand manner. At times it must be left alone altogether; since it may help the patient considerably, both by controlling the excessive ebullition, and by carrying off the disease critically. In reality, it does little good to check this symptom at all; unless it has either gone on for some little time, or blood has been drawn from the arm. This must be remembered."†

These extracts may suffice to illustrate the *methodus medendi* by removal of the febrific poison, according to the natural process. Sydenham knew little of antidotes to these poisons; nor were the so-called "febrifuges" anything more than means used for moderating the febrile symptoms; but convinced, himself, of the utility and safety of the Peruvian bark, he spoke out boldly and wisely in opposition to the prejudices of his contemporaries. He frankly declared his inability to explain its efficacy, but he maintained that previously to the discovery of its uses, agues were justly called the *opprobria medicorum.* No remedy has so constantly maintained its position, so that at this day it is the most available *antidote* we possess to the malarious poison; and during the last few years it has been tried, and with reported success, in the typhus class. The extent to which this fever-antidote is administered in the United States, as a cure for all forms of intermittents, has no parallel in this country. Dr. Wood informs us, that from thirty to sixty or even one hundred grains have been given during twenty-four hours, although he thinks from twelve to twenty-four grains is all that is necessary to be given between the paroxysms. Nor is the occurrence of local inflammation, if it be of a low type, a contra-indicant. "When typhoid pneumonia supervenes upon, or becomes complicated with, intermittent fever, it will be proper not to wait for a distinct intermission, but to administer the quina as soon as the nature of the disease is clearly ascertained. I have seen," says Dr. Wood, "the happiest effects result from this treatment, and have been informed by physicians residing in miasmatic regions, that they habitually employ it with great advantage. Not only is the intermittent interrupted, but the inflammation itself puts on a more favourable character." Equally beneficial results follow the use of this antidote in intense bilious remittents. "When a paroxysm of great virulence has occurred, from which the patient has been saved only by the most strenuous exertions, and there is every reason to fear that a similar one will prove fatal, recourse should be had to the sulphate of quina in the remission, however imperfect or short it may be. . . . . I am entirely confident that I have seen lives saved by this treatment, which must have

* Op. et vol. cit., p. 56. † Ibid., p. 64.

been inevitably lost under any other." The great contra-indicant is cerebral congestion or inflammation, for the antidote acts powerfully on the encephalon. In the 'pernicious' form of malarious fever, in which the poison acts so intensely on the nervous system that the paroxysm is not unlike an attack of epidemic cholera, the sulphate of quina is the only known antidote, and the sheet-anchor in treatment. "As soon as a remission or intermission has been obtained, there is but one course of treatment, and that is all important. . . . . No matter whether the patient has been under treatment during the paroxysm or not; no matter how partial the remission, provided it be a remission; no matter at what period of the interval the practitioner may have been called; his first, his last, almost his only thought, should be sulphate of quina. This is the remedy for the disease, and only this. . . . From thirty to sixty grains of this salt should be given, from the commencement of one paroxysm to that of the next." The experience of the French physicians in treating the paludal fevers of Algeria in every way corroborates these views.* But what is more remarkable is, that the remedy has been given with success in the ochletic yellow fever. At New Orleans from fifteen to thirty grains of the sulphate are given at once, combined with adjuvantia and corrigentia suitable to the particular case, and the same or smaller doses repeated if necessary. The febrile action is said in many to subside very speedily, and the patient frequently to enter immediately into convalescence; but in other cases it only modifies the phenomena, and the disease marches steadily onward to black vomit and a fatal issue. Cases are also reported in which the quina has cut short an attack of abdominal typhus, and as the former is only, we think, a variety of the latter, the statements on this point are worthy consideration. It must be remembered, however, that the fevers of this group are complex, and it may be readily granted, that while the *malarious* element of the compound poison may be counteracted, the *fæcal*, or *ochletic*, or specific fever-poison may remain uninfluenced. In this way we can explain the powerlessness of quina in those specific forms of yellow fever which should be classed with the exanthemata, and in all others of this group, as exanthematous typhus, variola, &c. It will perhaps be found useful in some degree as an antidote to the fæcal poison; but still, even if powerless over the miasmatic element, the counteraction of the malarious is a great point gained in the complex-fever group: it would be better, however, to give the quina as a prophylactic rather than a curative agent. There is a destructive class of infectious and contagious fevers, especially those of ochletic origin, as epidemic cholera and yellow fever, which derive much of their fatal energy from the previous entry of the malarious poison into the blood, so that the way to disarm them of their virulence and render the attack milder and safer, is to administer quinine as a prophylactic. This plan we adopted during the cholera epidemic with marked success.

The experience of the antidotal properties of quina should encourage practitioners to seek for an antidote for the fæcal and ochletic miasms, and for the specific contagions, since the probability that such antidotes exist is demonstrated by that experience. Two centuries ago, an antidote for the malarial poison was much less probable. "Specific medicines," Sydenham remarks, "in the restricted sense of the word, are by no means of every-day occurrence. They do not fall to every man's lot. Nevertheless, I have

* Vide vol. x. of this journal, p. 362.

no doubt, but that out of that abundant plenitude of provision for the preservation of all things wherewith nature burgeons and overflows (and that under the command of the great and most excellent Creator), provision also has been made for the cure of the more serious diseases which affect humanity, and that near at hand and in every country."*

The fact that so many persons are exposed to the specific poisons without injury or any apparent impression whatever on their health, merits close investigation; for it cannot be denied that at least hundreds of medical practitioners receive them into their blood daily without harm. The discovery of the mode in which nature protects would be a great triumph of art. It ought not to be forgotten, in all inquiries into contagion and infection, that the blood of a healthy person may not only be a fomites, but for anything known, there may be the same rapid generation of the poison as in the sick, only with an equally rapid elimination. Nature is undoubtedly equal to the task.

After all, the greatest triumph of art would be the utter removal of the sources of these fatal poisons. In a system of public hygiène is to be found the great means of prophylaxis; but even in the adoption of hygienic means the great protecting instinct has anticipated man's highest wisdom. The instinct of personal cleanliness in animals is nothing more than another manifestation of that same guiding principle which provides for the depuration of the blood; and it is curious to watch how, in animal communities with social instincts, a system of public hygiène is developed. Thus the bees of a hive provide for its efficient ventilation by mechanical means, as perfectly applicable to its purpose as it is ingenious; or if the dead body of an enemy cannot be removed from the hive, they provide against the evolution of putrescent effluvia from it by sealing it up hermetically in a coffin of wax. Numerous similar illustrations of instinctive, social, and personal hygiène are afforded by natural history. It has been observed, for example, that certain of the feline carnivora adopt exactly the same method of disposing of fæcal excreta which was enjoined by Moses on the Jews in the Wilderness, with the view of preventing dysentery breaking out in the camp. So it is that in the smallest and most trivial, as in the greatest and noblest, of God's works, there is the same incessant operation of a beneficent principle; it supplies the example which man can most safely follow, and its works are those which it is the height of human knowledge to know. It is very sure, that in medical science and art, as in all others, the grand principle of Bacon is strictly true: "Homo, NATURÆ *minister et interpres,* tantum facit et intelligit quantum de NATURÆ ORDINE re vel mente observaverit; nec amplius scit aut potest."

Having in some degree indicated the method whereby a modern humoral pathology, based on vital dynamics, may be made available to the better illustration of the pathology of fevers, and to the discovery and perfection of better methods of treatment, we will turn to that other large class of blood-diseases dependent primarily upon changes *within* the organism, and of which gout and rheumatism are the great typical forms. By the phenomena of this group, the relations of the humoral pathology to the laws of development, to histology, and to bio-chemical forces, are best illustrated. By selecting them we can also compare the two systems of Wood and Wunderlich. Dr. Wood divides his pathology and therapeutics into

* Op. cit., vol. i. p. 22.

two parts, the first, *general*, the second, *special*. The special pathology is divided into three classes—namely, general diseases, or fevers proper; constitutional diseases, or gout and rheumatism; and local diseases, or diseases of organs, or systems of organs. This arrangement is, we need hardly say, confessedly empirical, for Dr. Wood by no means denies that struma and other similar diseases are not constitutional; he excludes the latter simply as a matter of convenience. Wunderlich grapples at once and successfully with the difficulty from which Dr. Wood shrinks, and this wholly by applying the principles of the humoral pathology. According to his arrangement, so far as it is given in the part before us, the constitutional affections are as follow:—(A.) Fevers.—(B.) Dyscrasiæ, not caused by a specific cause: in this are included—1. Those dependent upon morbid nutrition.—(i.) General cachectic state; (ii.) Marasmus, or tabes; (iii.) Chlorosis; (iv.) Struma; (v.) Polysarcia, or fatty degeneration.—2. Those characterized by a tendency to hæmorrhagic effusion: (i.) Scorbutus; (ii.) The transient hæmorrhagic diathesis; (iii.) Morbius Maculosus (Werlholfii); (iv.) Habitual hæmorrhagic diathesis.—3. Those characterized by abnormal excretions or exudations: (i.) Dropsy; (ii.) Azoturia; (iii.) Melituria (Diabetes mellitus); (iv.) Icterus; (v.) Pyæmia; (vi.) Ammoniacal excretion (uræmia), suppression of urine. To this group Wünderlich would add, general tuberculosis, general cancerous degeneration, diphtheritic products, with phosphatic and the so-called arthritic deposits; but he proposes to treat of all these subsequently.—(C.) Constitutional diseases, characterized by structural changes in special tissues or organs. This includes: 1. A group in which the changes are induced by specific agents, but without reference to the close connexion of the structural with the functional disorder, and which comprises the metallic or mineral cachexiæ—namely, those induced by lead, copper, mercury, arsenic, phosphorus, and iodine.—2. A group caused by the entrance of vegetable substances, and which (rather paradoxically) has for its first species alcoholismus, the second being the opium cachexia. With the consideration of this uncompleted, the part before us ends. The fragment we have given suffices, however, to show the more comprehensive character of Wunderlich's arrangement.

Gout, according to Dr. Wood, is a constitutional affection, exhibiting itself in a peculiar irritation or inflammation in various parts of the body, of which, probably, no one vital portion or tissue is at all times exempt. He considers it under the three heads of *acute*, *chronic*, and *nervous gout*. As to the nature of the constitutional affection, he remarks:

"The humoral pathology taught that the phenomena of gout were owing to a peculiar peccant matter existing in the system, which it was the business of the paroxysm to eliminate. This opinion, abandoned after the times of Cullen, is again gaining ground, and ranks at present among its advocates men of high name in the profession, of whom not the least conspicuous is Dr. Holland. The fact which lends the strongest support to this opinion, is the tendency evinced in gout to the excessive production of uric acid. Not only is that acid thrown out in excess in the urine, giving rise to frequent lateritious sediments, but it is also often deposited in the joints, or their vicinity, in the state of urate of soda or of lime. Dr. Garrod has proved, by chemical examination, that the blood in gout contains urate of soda, which can readily be separated from it in a crystalline state. He has also shown, that before the occurrence of the gouty paroxysm, there is, along with this excess of urate of soda in the blood, a deficiency of uric acid and its salts in the urine.

The use of an excess of animal food, with insufficient exercise, which disposes to an excessive production of uric acid, predisposes also to gout; and the uric-acid lithiasis, or gravel, not unfrequently alternates with that complaint. But these arguments, as well as others, drawn from the moveable characters of the local disease, as if it depended upon an offending matter carried everywhere with the blood, and from the relief experienced after the paroxysm, as if the offending matter had been discharged, appear to me to be inconclusive. Uric acid, I think, certainly cannot be the matter in question. This substance is generated in excess, and thrown out by the kidneys in many diseases in which no sign of gout is exhibited; and cases of gout often exist, especially in its irregular forms, in which there is no evidence whatever of an excess of uric acid; nor can I exactly perceive how the production and elimination of this matter, if not essential to gout, should lend any support to the idea of the existence of some other unknown and concealed matter, which is the real offending cause. It is possible there may be such a substance; but its existence, not to mention its nature and properties, has certainly never been demonstrated." (vol. i. pp. 479, 480.)

Dr. Wood might have added, that colchicum, the specific for gout, increases the quantity of uric acid in the urine, as shown long ago by Chelius. The paragraph is a good example of Dr. Wood's style; he states the facts succinctly and fairly in support of a given doctrine, and when you expect the conclusion as a matter of course, some indefinite negatives are advanced to meet the positive facts, doubts are expressed as to the facts themselves, and the reader is pushed into a dark closet of inferences, to find his way out as he best can, or accept the indefinite conclusions to which the author has led him. Let our readers compare Dr. Holland's lucid pathology of gout, and especially his general deductions, with the following conclusions of Dr. Wood:

"All of a general nature that we can fairly deduce from the symptoms is, that there is a morbid state of the system which probably involves all its essential constituents, and which evinces itself now and then by peculiar local phenomena, which may be either purely nervous, purely functional, or inflammatory. That the inflammation is peculiar or specific is proved by its shifting character, and indisposition to the secretion of coagulable lymph or pus." (vol. i. p. 480.)

Or, in other words, we know nothing about gout. Now, this is so far from being the necessary deduction from recorded experience, that we would almost venture to say, that there are few constitutional diseases that are better understood, even if we go no further than the facts as stated by Dr. Wood. It suffices to remark, in answer to Dr. Wood's negative arguments, that the excessive production of uric acid is by no means necessary to induce gout. An excessive production is a characteristic of the gouty *diathesis;* but so long as the acid is freely eliminated, the individual is free from *gout*, just as he would escape the effects of other poisons, which are harmless if carried off as rapidly as received. Mercury must be prevented "running off" by the intestinal canal if we would induce its constitutional effects; and comparatively small doses will produce febrile action and local inflammation (ptyalism) if the whole that is administered be retained. If there be no evidence of lithic acid in the urine of persons of a gouty constitution suffering from gout, we may conclude that it is retained in the blood, not as uric *acid*,—that would be a conclusion not warranted by the facts,—but possibly as a *urate* of soda. It must be remembered, also, that the sudoriferous glands take on a vicarious

action in the gouty, and are auxiliary to the kidneys. It is, however, to be noted, that Dr. Wood adopts the humoral doctrine in practice, although he rejects it in theory.

"The paroxysm in gout is certainly not the whole disease. There is undoubtedly a morbid state of system to which the paroxysm is owing, and which it has a tendency to relieve, if allowed to run its course. It is no less true, that the removal of the paroxysm has no effect in removing the state of the system alluded to. If, therefore, by remedies addressed exclusively to the former, we succeed in cutting it short, we may very possibly leave the latter still in existence, and ready to display itself by some assault, it may be upon one of the joints as before, or it may be upon one of the interior or vital organs. The most prudent procedure, therefore, would appear to be, to allow the inflammation of the joint to complete its course, contenting ourselves with moderating the violence if excessive, and endeavouring to render the patient as comfortable as possible." (*loc. cit.*)

This is just the old prescription of "flannel and patience." Sydenham's advice is not widely different from Dr. Wood's. "The elimination of the *causa continens* is," he observes, "the work of Nature, and it must be done according to Nature's own method." Again:

"In gout, however, it seems as if it were the prerogative of Nature to exterminate the peccant matter after her own fashion, to deposit it in the joints, and afterwards to void it by insensible perspiration. In gout, too, but three methods have been proposed for the ejection of the *causa continens*—bleeding, purging, sweating. Now none of these succeed."*

He discusses the three methods, and shows how it is they fail. Purgatives, for example, cause the peccant matter meant for the joints to take hold of the viscera.

"Nature, diverted from her own good and safe method of depositing the peccant matter in the joints as soon as the humours are solicited towards the intestines, instead of acute pains with little danger, induces sickness, griping, fainting, and other irregular symptoms, which will nearly destroy the patient."†

It is not possible to watch an exquisite case of gout—that is to say, a case, in which all the most striking characteristics of the disease are manifested—without coming to the conviction that it is blood-disease. Nothing is more extraordinary in pathology than to see the chalky-looking matter (the urate of soda) flowing from the ulcerated bursæ mucosæ situate about one or more of the joints (as the elbow, wrist, &c.), and to note with what sensible benefit to the sufferer the discharge continues. Although he be crippled in every limb, advanced in years, and unable even to move in bed, all the vital functions go on well so long as there is a free exit for the chalk-like matter. Being a blood-disease, the pathology of gout is to be considered as a branch of pathological toxicology: and just as fevers illustrate the acute and chronic blood-diseases arising from poisons derived from without; so gout illustrates a large group of similar diseases arising from poisons derived from within. As in the former, so in the latter, we have to consider the blood-poison etiologically; then the results of its action on the organism generally, in inducing a curative reaction; and on the organism locally, as either acute or chronic affections. In considering these points, Dr. Wood's division into acute, chronic, and nervous gout may be adopted.

The decomposition and recomposition of the histological constituents of the organism is the final cause of the whole mechanism; it is the essence

* Op. cit., Treatise on Gout, p. 131. † Ibid.

of vital action. The bio-chemical forces by which this process is effected have been sufficiently elucidated, to establish at least the general fact that the effete or used-up matter passes out of the body in the form of *excreta*, and constitute the various nitrogenous, hydrogenous, and other compound inorganic products given off from the excreting surfaces of the kidneys, skin, lungs, and glands in connexion with the gastro-intestinal canal. By far the largest supply of excreta must be sent into the blood from the *motor* apparatus, whether voluntary or involuntary, and by the co-ordinatory apparatus thereof—the nervous system. This is the necessary result as well of the extensive histological surface as of the degree of vital action. The exact result of the bio-chemical changes going on is not yet determined, but it is generally allowed that part is elaborated as carbonic acid from the lungs, and part as urea and other urinary excreta from the kidneys. It is very probable that the skin also acts as an excreting organ, but to what extent and under what circumstances has not yet been ascertained by physiological chemistry. The facts of pathology render it certain, however, that it takes a larger share than is usually allowed. The acid and fœtid excreta of the skin which constitute such striking symptoms in some diseases, have had little or no light thrown upon them by physiology, except what is included in the general principle, that one part of the great system of excreting surfaces may take on the functions of another part, when the adaptive principle of the organism indicates the necessity. In this way fœtid excreta from the tonsils, the pulmonary mucous surface, the skin of the feet, &c., may be considered to depend on a vicarious action eliminating those compounds of sulphur, or phosphorus, or carbon, with hydrogen, usually transmitted through the intestinal glands. So also the acid sweats in rheumatism, miliary fever, &c., may be due to the elimination of effete materials, which, under other forms, should have passed off by the liver or kidneys. Amidst all this uncertainty there is no doubt but that the effete residue of the histological transformations in the muscular and nervous systems have their outlet, to a great extent, by the kidneys, and that in gout and gouty diseases, these transformations and the elimination of the products are of primary etiological importance.

The excreta enter the blood only to be eliminated. This process of depuration goes on incessantly, for the necessity is as continuous as vital action itself. If they accumulate in the blood faster than they are eliminated, they excite the protective principle into activity, and special means are used for their elimination. In this way retained excreta induce febrile action, the intensity of which will be in relation to the amount of the poison accumulated and the force necessary for its elimination. It is thus that checked perspiration is supposed to induce febrile movements. The amount of the excreta-poison accumulated will depend upon two principal circumstances—namely, the rapidity of its production and the relative activity of function of the organ appointed to eliminate it. Applying these views to urea and the urinary salts, and adopting the theory that they are poisonous and induce disease (as well those of a gouty character as others), and are to be eliminated by the skin and kidney, it is evident that there may be an accumulation from so excessive a transformation of the nutrient materials, that the kidneys or skin, although healthy, are not equal to their elimination; or that there may be only a normal, or even

an inferior activity of transformation, but that an accumulation supervenes from functional or structural disease of the kidneys or skin, whereby the elimination is impeded or arrested. Such being the probable facts as to the origin of the poison (or peccant matter), it is obvious that the pathology of gout and of renal disorders generally must be considered from a much wider and more comprehensive point of view than hitherto. We at once see the necessity of inquiring more particularly into the nature of these transformations, into the functions of the viscera and tissues in which they take place, and as to the operation of the retained poison upon the tissues from which it is derived. We get a glimpse, also, of an entirely new nosological arrangement, and see how it is possible to include a larger number of apparently widely dissimilar diseases in one great group, comprising morbid transformations of the vascular and muscular systems.

It has been already noted as a general fact, that fever poisons received into the blood from without have a special local action, and that it is the tendency of those derived from animal excreta to exercise a morbid influence first upon the blood and tissues, and then, or concurrently, upon the excreting surface from which they are normally eliminated. Thus the fæcal miasm induces diarrhœa and dysentery; the *ochletic* causes pulmonary, pharyngeal, and cutaneous inflammation. If we apply this general principle to the urinary excreta, we should conclude that they would act injuriously, first upon the tissues, from contact with which it is a primary principle of the organism to remove them; and secondly, or concurrently, upon the excreting organs allotted for their removal. The latter are the kidneys or skin; the former are principally the motor apparatus. The motor apparatus differ in their intensity of action and their extent of surface. The voluntary system is the most extensive, but it has long intervals of inaction; the cardiac involuntary system, less extensive in surface, is more intense in action, because it never ceases. The pulmonary is next to it in degree, and then the abdominal tubular muscles. We should therefore infer, *à priori*, that the accumulation of renal or cutaneous excreta in the blood, would manifest itself in histological change of structure, in modification of function, and in capillary derangement of those organs and tissues in which the production of those excreta is most active, or from which they should be eliminated. This is, in fact, what occurs in gout, in rheumatism and rheumatic gout, in arthritic and rheumatic inflammation by metastasis, and in those affections of the kidneys and skin which impair or arrest their eliminating function. The voluntary muscular system and its appendages, the heart, and vascular system, and the kidneys and surface, are, in all these diseases, the seat of the pathognomonic phenomena. It was on these general grounds that we were led to notice and teach the connexion between uræmia (caused by Bright's disease) and the inflammations of serous membranes which are developed during its course, for some years before that connexion was so ably demonstrated by our late lamented friend, Dr. J. Taylor. It is probable, however, that the poison which excites gouty erysipelas or chronic gouty eruptions, differs from that which gives the acid, or straw-like odour, to the perspiration in rheumatic fever, miliary fever, syphilis, &c., and it is obvious that, in addition to the isomerism of these excreta, and the facility with which their constituent elements are recombined so as to mask their true character, there is the

difficulty of ascertaining the chemical relations of the cutaneous excreta in consequence of the impossibility of collecting them for analysis. Nor could the determination of the urates to the bursæ mucosæ and tendon-sheaths be easily explained, even on the principle of vicarious action, until lately, since good evidence of a secreting structure was wanting; but Mr. Quekett has demonstrated (what had been already hypothetically inferred from these pathologieal phenomena) that these tissues are in their histological structure identical with secreting surfaces, having a series of processes like villi upon their surface, which project into all parts of the cavity of the joint or of the sheath, so that the true nature of the articular inflammation in gout is now more apparent. In support, therefore, of the opinion that it depends upon the local action of urinary constituents retained in the blood, we have, in addition to the ample evidence of accumulated experience, the philosophy of vital dynamics, improved chemical research, the general doctrine of vicarious action, and the most recent and trustworthy researches into histological structure.

These views place us in a position to ascertain the relations of the antecedents to the arthritic condition of the blood and the tissues, and to consider the phenomena by which it is manifested. The two great predisposing antecedents are high living and an hereditary conformation of the system. The relations of the former are sufficiently obvious; those of the hereditary state are more obscure. Experience establishes this, that the gouty predisposition shows itself in two classes of persons, the one comprising those with bilious, sallow complexions, small limbs, feeble vitality, tolerant of stimulants, and subject to asthenic forms of disease. The other group, and by far the most numerous, includes persons of a sanguine temperament and portly figure. These have large limbs, broad thorax, firm, regular, well-set teeth, florid complexions indicative of a highly developed capillary system, and a blood rich in globules. Advancing years modify the complexion, for hepatic disorder is apt to give a sallow tint (leaving, however, the characteristic mark of numerous small bloodvessels meandering over the cheek), and fat becomes deposited, so that corpulence shows itself; in many the hair becomes grey early, not however falling off from the crown even at an advanced age, and the teeth remain sound until late. As regards the general habits, the most noticeable are mental and bodily energy, and a love of "the pleasures of the table," rich food and wine being desired and enjoyed. The pathological conditions are urinary deposits of lithic acid, renal diseases, and analogous transformations of the serous and sero-fibrous tissues. In particular, the left heart, its valves, and the arterial system, undergo patchy calcareous degeneration or spurious ossification, causing cardiac diseases of various kinds, and consequent pulmonary congestion, with its sequelæ, hypertrophy and dilatation of the right ventricle, hepatic and renal congestion, and sanguineous apoplexy. This is a general description of the most *typical* form of the arthritic diathesis, and in such gout in any of its forms may be readily developed, whenever the appropriate exciting causes are brought into action. There are numerous instances of a latent arthritic diathesis in which the temperament is not purely the sanguine, but compounded with states in which the nervous system is highly developed, or there is a strumous taint, or a leading predisposition to hepatic disorder. These may modify the general characteristics con-

siderably; nevertheless the practised eye can detect the latent arthritic conformation.

It is an important point in the history of gout that it rarely attacks females, although we know instances in which girls under twenty, with a strong hereditary taint, have suffered. Nor are men affected until the middle of life, except in young men (as in girls and women) with a very decided hereditary tendency. It is probable that when it occurs under unusual circumstances as to age or sex, the principal predisposing cause is the too great or rapid transformation of the tissues, but that when it occurs (as is usual) about the age of forty, the predisposing cause is to be found, mainly at least, in imperfect renal action. The great dependence of this function upon due innervation of the kidneys, is probably one of the reasons why excess in sexual indulgence, as well as excess in eating or drinking, is so often followed by an attack of gout. It is quite certain, too, that ovarian influence modifies the renal function very considerably in females with a gouty predisposition, and although a paroxysm of gout, or even articular inflammation, is very rarely induced thereby, yet one or more of that group of diseases termed nervous gout is induced, and the anomalous forms of hysteria excited. This doctrine, as to the *ovarian origin* of arthritic hysteria, which we promulgated thirteen years ago, has made progress of late years, in proportion as the modern humoral pathology has been developed, and is now fully adopted by eminent physicians. Its best example is that state of the system which we termed *neuræmia*.*

The *exciting* causes of a paroxysm of gout (the acute form) do not differ essentially from those of fevers. Exposure to cold or other depressing agencies, as watching, fatigue, and the operation of other febrific poisons on the blood, are some of these. The points more particularly to be noticed in the paroxysm are, firstly, that the pain is essentially neuralgic, although aggravated, doubtless, by the local inflammation; secondly, that the inflammation is specific; thirdly, that the phenomena are periodic. This last point is little noticed by later systematic writers, yet it is of considerable importance in determining the treatment, for we learn thereby that the paroxysm (like an attack of fever) will have a natural termination. It is, in fact, a fever of the tertian type, lasting fourteen days; it is nevertheless eminently a recurrent fever, for there is a constant reproduction of the febrific poison, and the organism appears never to become habituated to its presence in the blood. Hence if its elimination be prevented, the febrile phenomena recur, but with each recurrence, especially if they be frequent, there is less power of reaction, less perfect elimination by the natural process, and, finally, the chronic disease.†

* The doctrine of a gouty hysteria is not new. "Of this gouty kind, also (observes Dr. Cheyne), is the nervous Sharpnesses of hysterick Women, which sometimes is a Pain in the *Forehead*; a violent Stitch and Pulsation, fix'd over one of the *Eyes*; a Coldness in the *Top* of the Head, on the *Temples*, and almost all other Parts of the Body by Turns, a Pain in the *Teeth*, the *Jaws*, the *Stomach*, the *Sternum* and the *Guts*, is a *Strangury*, *Tenesmus*, a *Colic*, the *Gout*, and the Gravel. In short every Thing."—An Essay on the True Nature and Due Method of Treating the Gout. By Geo. Cheyne, M.D. and F.R.S. 7th Edition, 1725, p. 130.

† "Hence likewise we may see the Reason, why the first Fits, of otherwise healthy Persons, enrage and remit once in four and twenty Hours. The Access being at Sunset, and the Remission about Daybreak: why the whole Fit in such lasts about fourteen Days. And lastly, why the particular Accesses and Remissions of Persons advanced in Years, and broken with the *Gout*, happen only once in fourteen Days. And why the whole Duration of the Fit is made up of several such Accesses and Remissions in equal Times."—Cheyne in *op. cit.*, p. 7.

In treating paroxysmal gout of the normal type, we inquire, first, what is the antidote to the poison? secondly, through what organs does nature eliminate it? It can hardly be conceived that it is eliminated into the seat of inflammation, except in those instances in which concretions form, or the chalky fluid is poured out from the suppurating surface of the ulcerated bursæ and tendinous sheaths. There are some good grounds for the opinion that the cutaneous surface as well as the kidneys gives exit to the poison. Of the two, the skin is certainly the safest point for artificial aid, for the kidneys participate already in the morbid action, and are indeed, possibly, its primary seat; but even powerful diaphoresis is not quite safe during the paroxysm, however valuable in the interval. The maxim of the old humoral pathologists is very applicable to gout, and should never be forgotten in treatment, whether we wish to excite the local inflammation in a joint as a counter-irritant to metastatic inflammation, or as prophylactic means when such inflammation is feared, or whether we wish to cut short the fever by rapidly eliminating the poison. In all these, "*Ubi irritatio ibi fluxus*" is a cardinal maxim. As to the antidote to the poison, the most approved is colchicum, although the ranunculaceæ have been used. In some instances quinine is very available, more particularly when a malarious poison is co-existent. There is much truth in Dr. Wood's doubts as to the *modus operandi* of colchicum, and as to the amount of our knowledge on that point. Dr. Wood states it has no decided narcotic property, and therefore cannot act as an anodyne; and in the ordinary sense of the word, this may be true. It is very possible, however, that it has a sedative effect on the *vital actions going on in the tissues themselves*, so that the rapidity of the transformations is diminished, and in this way the generation of poison is stopped. For the same reason, it may facilitate renal and cutaneous excretions. The free use of diluent fluids may (as in other fevers) accelerate the elimination of the *materies morbi*. It is in prophylaxis that colchicum may be given most safely; administered in small doses, so as not sensibly to affect the bowels and in combination with chalybeates, the haloid salts, or such other remedies as the special habitus of the individual may indicate, it is undoubtedly a very valuable remedy in almost every form of chronic or masked gout.

The *cutaneous* gouty diseases have scarcely had so much attention directed to them as their importance merits. Their true nature is overlooked by the practitioner, unless there be an efflorescence or deposit of urates, or the presence of the arthritic diathesis arrests his attention; for they will occur in gouty persons who have never had a paroxysm. Prurigo, and the desquamative inflammations of the derma (as psoriasis inveterata), are the most usual forms. Although concretions of urate of soda have been found in the mammæ, the ears, and as a saline efflorescence, on the skin of gouty persons, it is by no means certain that the excreta eliminated are compounds of urea; in some instances they have been suspected to be oxalates. The determination of the urinary constituents to the derma is an interesting pathological fact, because the derma is to be considered but a portion (inverted) of the great gastro-intestinal system. Until lately, this was only an hypothetical deduction from the general principles of embryology and morphology; but Kölliker, having established the existence

of the non-striated class of muscular fibres in the integument, has set this point at rest.*

Urinary salts have been discovered in the saliva, and all analogy would lead us to the inference that they may appear vicariously in any of the inner mucous surfaces as well as in the skin. In this way we comprehend gouty coughs, gouty asthma, colic, and various other spasmodic diseases arising in the course of chronic gout. Patients of this class are also subject to gouty flatulence, when vast volumes of nitrogen are given off. Wunderlich thinks it is derived from the air which has been swallowed with the food, and not an excretion from the gastric mucous surface; but this explanation is not sufficient to account for the enormous quantity poured out in a very short time in cases of this kind, and it seems more reasonable to infer that it is due to some bio-chemical action on the urinary excreta, which are for the most part rich in nitrogen.

Nervous gout is as proteiform as hysteria; there is, indeed, no functional disorder of the nervous system which may not appear under this form. In this respect the arthritic poison is closely analogous to the malarious, which induces a great variety of nervous disorders, from simple neuralgia to coma and catalepsy. The extreme grade of morbid action in urinary poisoning is seen in the coma, epilepsy, or amaurosis, developed during the progress or towards the termination of Bright's disease. The most rapidly fatal is that in which extreme mental labour determines the inflammation or irritation in a paroxysm of gout to the cerebrum, when furious delirium, mania, or encephalitis supervene. Neuralgia is an early symptom in Bright's disease. Very rightly Dr. Wood impresses upon his readers the necessity of tracing out the arthritic relations of the patient in all cases of obstinate neuralgia. Probably lead, malaria, and the urinary poison, are more frequently the causes of neuralgia than all others together; and when we consider the general pathology of neuralgia, it is to be feared that hypochondriasis and hysteria are not the only cerebral (mental) disorders induced by the gouty blood, but that even some of the more unmanageable forms of insanity may be referred to this cause.

There is one other important point in the pathology of gouty disorders, which a humoral pathology, conjoined with histological research, may strongly elucidate. The two great series of tissues affected with gouty disease are also subject to transformations and degenerations, under circumstances which point out their relationship to the same primary morbid condition. It is a well acknowledged fact, that the hereditary predisposition to gout is little amenable to the same methods of treatment as the acquired; but beyond this, our knowledge is imperfect. We know nothing of that congenital condition of the germ and of the nutrient *nisus* on which the predisposition depends, but that it is closely connected with some more recondite condition than is suspected, seems to be indicated by several facts. In the first place, the greater development of that predisposition in males is not, probably, to be attributed wholly to habits of life, or to the physiological influence of the sexual organs, but rather to the same vital force which determines the difference between the germ-cell and the sperm-cell. There is a condition of the bloodvessels, for example, which is hereditary,

* Vide vol. ix. of this journal, p. 210.

but strictly limited to males, although *transmissible* through the female; in these cases the hæmorrhagic diathesis seems to depend on imperfect contractility of the vessels. Angina pectoris is a disease of the vascular system, traceable to the gouty diathesis, but manifested almost exclusively in males: it has lately been attributed to fatty degeneration of the cardiac fibres, as well as to calcareous transformation of the coronary arteries; in either case the histological morbid condition and the sex are not without some relation. Fatty and calcareous degeneration in the muscular system is most usually met with in males. Mr. Quekett mentions a most instructive instance of this, in which, of a family of nine children, the six girls were perfectly healthy; but all the boys, on arriving at the age of three or four years, began to lose the use of their limbs. The disease was inexplicable, until one of these (the eldest) having died, the muscles were found to have undergone fatty degeneration, the brain and spinal cord being perfectly healthy.* It is not altogether irrelevant to notice another circumstance—namely, that fatty degeneration of the kidney in men is not unfrequently associated with the gouty diathesis, or with chronic gout, and that a general tendency to deposit fat (or corpulence) is associated equally with an hereditary and acquired gouty habit. Whatever may be the theoretical value of these facts, they have undoubtedly a practical bearing upon the treatment of the entire group of diseases in which there is abnormal nutrition of the tissues, inasmuch as they point out new relations to functional and structural diseases which are in close relation to the products of nutrient transformation. Nor is the theoretical value small, for in this direction—namely, the combination of vital dynamics with histological and biochemical research and pathological experience—lies the way of escape from the vicious circle to which medical science and art have been so long restricted.

As an opinion may be expected on the value of the works before us, we can confidently state that they may both be considered standard works, and interesting to the English practitioner as treatises presenting diseases under new aspects. Dr. Wood's cannot be compared, however, with such a work as Dr. Copland's, even as a compendium of the best current medical doctrines and modes of treatment. But it has gone into a third edition, and this is praise enough. In the present edition the additional pages are principally occupied with the relapsing fever described by Dr. Jenner; the dengue (unaccountably omitted in the last edition, and having points of resemblance to the relapsing fever); certain cutaneous affections, as trichosis, pellagra and lupus; and the more recent views as to Bright's disease, hæmorrhages, &c. We have not given, nor attempted to give, any abstract of the matters contained in either of these authors; we felt, indeed, that such a method of criticism is not applicable to systematic works, if it were possible to adopt it with satisfaction either to the author or the reader, which it is not. It seemed to us a more useful method to compare, critically, current pathology with present physiology and past principles of treatment, so as thereby to determine in what respects we have advanced or can advance; in what respects we have remained stationary or even retrograded. We adopted Sydenham as the best modern

* Lectures in Histology, p. 197.

instance of the school which treats disease according to the natural method; and the extracts we have given (which might have been multiplied indefinitely from other writers as well as from Sydenham) will enable the reader to judge for himself in these matters. We cannot but think that more philosophical views will arise even in our day. The phenomena of disease (to use the words of Sydenham), "if carefully collated with each other, lead us, as it were, by the hand, to those palpable indications of treatment which are drawn—not from the hallucinations of our fancy, but —*from the innermost penetralia of nature.* By this ladder and by this scaffold did Hippocrates ascend his lofty sphere—the Romulus of medicine, whose heaven was the empyrean of his art. He it is whom we can never duly praise. He it was who then laid the solid and immovable foundation for the whole superstructure of medicine, when he taught that *our natures are the physicians of our diseases*—Νούσων φύσιες ἰητροί. . . . . . Herein consisted the theory of that divine old man. It exhibited the legitimate operations of nature, put forth in the diseases of humanity. The vain efforts of a wild fancy, the dreams of a sick man, it did not exhibit.*

T. Laycock.

Review II.

1. *Mikroskopische Anatomie; oder, Gewebelehre der Menschen.* Von Dr. A. Kölliker, Professor der Anatomie und Physiologie in Würzburg. 2ter Band.—*Leipzig*, 1852. Art., *Milz.* pp. 253.

Microscopical Anatomy; or, Description of the Tissues of the Human Body. By Dr. A. Kölliker, Professor of Anatomy and Physiology in Würzburg. 2nd Volume.—*Leipzig*, 1852. Article, *Spleen.* pp. 253.

2. *Ueber runde Blutgerinnsel, und über pigmentkugelhaltige Zellen.* Von R. Remak. *Müller's Archiv.*, February, 1852. pp. 115.

On Round Clots of Blood, and of Cells containing Pigment Granules. By R. Remak. (In *Müller's Archives* for February, 1852.) pp. 115.

Nucleated cells, from $\frac{1}{2250}$th to $\frac{1}{740}$th of an inch in diameter, containing in their interior a number of blood-corpuscles, varying from one to twenty,—such are the bodies on the alleged existence of which in the spleen considerable weight has lately been laid in reference to the function of that organ. Of the signification of these blood-corpuscle-holding cells, as they have been called, two opposite interpretations are given. According to the one, the contained blood-corpuscles are *new* formations which become free on the solution of their parent cell. According to the other, on the contrary, the contained blood-corpuscles are *old*, and about to be destroyed; and preparatory to this, they are presumed to have the cell wall formed around them.

The latter is the opinion enunciated and advocated by Professor Kölliker, who has especially taken the doctrine of blood-corpuscle-holding cells under his protection.

As the first view of the signification of blood-corpuscle-holding cells has been little worked out, we shall not consider it further. Confining our

* Op. cit., vol. i. p. 16. Preface to the Third Edition of Medical Observations.

attention to the second view, we shall have to discourse of pigment cells, as alleged phases in the history of blood-corpuscle-holding cells, and of extravasations of blood as the soil in which these bodies are said to be especially produced,—extravasations of blood not in the spleen alone, but in the brain, liver, and kidneys.

In extravasated blood, then, the distinguished Würzburg professor teaches, that the blood-corpuscles, at the same time that they become smaller and darker (and round in the case of the oval corpuscles of the oviparous vertebrata) agglomerate into small round heaps, which, with some blood-plasma, become, by the formation of a nucleus in their interior and a cell-membrane on their exterior, blood-corpuscle-holding cells. When thus enclosed, the blood-corpuscles are broken up and resolved into pigment granules of a gold yellow, brown red, or black colour. Pigment cells, met with in the localities and under the circumstances above indicated, are thus to be viewed as stages, on the road to destruction, of blood-corpuscle-holding cells.

In respect to the development of blood-corpuscle-holding cells, Professor Kölliker considers it certain, that they are formed, not directly round a nucleus, but by the deposition of a membrane around a small mass of coagulated blood, like the membrane around the last subdivisions of the fecundated yolk. Whether the nucleus, which is subsequently found in these cells, without exception, exists before the formation of the cell-membrane or not, he leaves undetermined, though seemingly disposed to think that it does, and that it is the *blast* or germ of the membrane.

It may be proper here to observe, that, along with blood-corpuscles, other matters may be included in the cells; for of blood-corpuscle-holding cells, met with in an extravasation of blood in the *commissura mollis*, Professor Kölliker mentions his having found some which contained *pieces of cerebral substance!*

But it would appear that it is not in extravasated blood alone that blood-corpuscle-holding cells are met with. In the *Triton*, Dr. Kölliker has found such cells within the capillaries of the semi-transparent spleen, arranged often in linear series, and admitting of being pressed onward into the veins, so that one of these vessels may frequently be found filled with nothing but such peculiar elements.* Whether this is of constant occurrence in the triton, Kolliker does not know, nor whether the same thing is to be met with in other batrachians. He can, however, affirm that in the triton, frog, toad, and black salamander, he has found blood-corpuscle-holding cells even in the trunks of the splenic and portal veins; and in Bufo cinereus, Triton igneus, and the salamander also, he has traced them in the hepatic branches of the vena portæ, as far as the capillaries of the liver. In the salamander he has even found them in the inferior cava and heart.

It was necessary, in the preceding account of blood-corpuscle-holding cells, to allude to their occurrence elsewhere than in the spleen; but the comments which we now proceed to make will have reference to them, only in connexion with their supposed subserviency to the function of that organ. And as introductory to this, we would beg the reader's attention to a brief outline of the intimate structure of the spleen, the result of an examination to which we subjected the organ some years ago.

To the naked eye, or to the eye assisted by a magnifying glass, the parenchyma of the spleen, as every one knows, is perceived to consist of two different substances: a red-pulpy looking matter, and the whitish Malpighian corpuscles.

The red pulpy matter presents indications of a subdivision into lobules, owing to the tuft-like arrangement of the vessels leading to and from the capillary network with which it is pervaded.

The Malpighian corpuscles, of a roundish shape, and about $\frac{1}{50}$th of an inch in diameter, lie imbedded in the red substance adhering to the smaller divisions of the arteries before they open into the capillary network, and not among the capillaries. In the spleen of the sheep, injected with chromate of potass and acetate of lead, the Malpighian bodies were not found penetrated by the coloured deposit, but they appeared plump and distended looking.

Examined microscopically, the red pulpy substance is found to consist of nucleated corpuscles and short nucleated fibres held together by a finely granular intercellular substance. Most of the nucleated corpuscles are destitute of an evident cell-wall; the nuclei, which are about the size of the red corpuscles of the blood, being merely surrounded by a granulous substance of irregular outline. Those corpuscles which possess a distinct cell-wall are about $\frac{1}{1800}$th of an inch in diameter. Besides their nucleus, which is similar to, though slightly larger than, that of the corpuscles destitute of cell-wall, these cells may or may not contain a little granulous matter. The nucleated fibres are fusiform, and generally straight, but individuals occur circularly coiled, the coil being maintained by a tenacious intercellular substance filling up the middle space. Besides the elements now described, larger corpuscles are met with, but in much smaller number, in the form, viz., 1st, of nucleated cells of about $\frac{1}{1000}$th of an inch in diameter, with a rather thick and not very smooth cell-wall, which resists the action of water, a large nucleus and some granules in their interior; 2nd, finely granular corpuscles of the same size, but in regard to which it cannot be very clearly ascertained whether they have a distinct cell-wall or not.

All the elements now enumerated are without colour. The proper colour of the pulp appears to reside in the intercellular substance, in which pigment granules are dispersed, partly free and partly contained in cells.

Examined with a low magnifying power, the Malpighian corpuscles present the appearance of thick-walled glandular vesicles, with contents. The thick walls are not defined and homogeneous, but are, on examination with a high power, found composed of nucleated fibres and nucleated corpuscles, similar to those of the red pulpy substance; between which, indeed, and the exterior surface of the Malpighian corpuscles, there is no very distinct line of demarcation, other than is produced by the condensation of the wall of the Malpighian corpuscles, and the absence in them of coloration.

The contents of the Malpighian bodies are nucleated granular corpuscles and nucleated cells, similar to those of the red substance, cohering together in a mass by means of a diffluent intercellular substance, and interspersed among them a few somewhat larger nucleated cells. We also observed a very pale capillary tube: but whether bloodvessel or not it was

not easy to say. Dr. Sanders and Professor Kölliker appear to have made a more decided observation of bloodvessels.*

The above description, which is in general agreement with the account given by Dr. Kölliker, was drawn up from the spleen of the sheep. In the human spleen we have not found the Malpighian corpuscles entire. We have found, in their situation among the divisions of the small arteries, merely a shapeless, soft, whitish substance, composed of the same microscopical elements as the Malpighian corpuscles. That this shapeless, whitish substance was the remains of softened and broken-up Malpighian corpuscles, appears from what may be observed in the sheep's spleen—viz., the existence of all intermediate gradations between this shapeless white substance and the still perfectly entire Malpighian corpuscle. The softened remains of the Malpighian corpuscles now passed into the red pulp without any line of demarcation as regards consistence; nor was there any longer a line of demarcation as regards colour. When it is remembered that the microscopical elements of the red substance and of the Malpighian corpuscles are the same, the softening of the latter may be viewed as indicating their resolution into the red substance.

Having thus examined the structure of the spleen, we next proceeded to a comparison of what enters with what issues from the organ—i. e., a comparison of the blood in the splenic artery with that in the splenic vein.

The blood in the splenic artery was found to be similar to that in other arteries; but the blood in the splenic vein was peculiar, inasmuch as it contained, besides the ordinary blood-corpuscles, a large number of nucleated corpuscles and fibres identical with those above described as composing the red pulp of the spleen, together with free nuclei similar to those of the nucleated corpuscles. On the occurrence of these nucleated corpuscles and free nuclei, the statements as to the great number of 'colourless' or 'lymph' corpuscles in the blood of the splenic vein appear to have been founded.† Numerous small masses of several of these corpuscles agglomerated together occurred in the blood, as well as in the broken-up pulp. These elements were traced as far as the vena portæ, but in the venæ hepaticæ they had mostly, though not entirely, disappeared.

The anatomical inference which might be drawn from the facts now related is, that some of the venous radicles of the spleen have a connexion with the red pulp of the organ analogous to that which exists between the radicles of the hepatic duct and the parenchyma of the liver; whilst the physiological inference is, that the materials thus derived by the blood from the spleen may concur in fitting it for the secretion of the bile.

To return to the question of blood-corpuscle-holding cells.—We have above seen that, according to Dr. Kölliker, blood-corpuscle-holding cells are met with in the batrachians, within bloodvessels. To this it is to be added, that they have been found, by Dr. Ecker, in the blood of the splenic

* We take this opportunity to mention that in a grayish pulpy matter, exactly similar in general appearance to the contents of the Malpighian corpuscles, which was evacuated from one of those vesicular granulations which are often found on the palpebral conjunctiva, in chronic ophthalmia, capillary vessels, filled with red blood-corpuscles, were discovered among the cells, in different stages of development, composing the matter.

† Funke. Ueber das Milzvenenblut. Henle Zeitschrift, Bd. 1. (Neue Folge) 1851.

vein of calves and swine sometimes, and of the horse once; in the blood of the splenic vein of which animal also, a single one was once found by Dr. Funke, who had before frequently met them in the spleen itself. We shall not, however, enter into the consideration of such intravascular cells, further than to remark, that supposing blood-corpuscle-holding cells to be formed in the parenchyma of the spleen, they will as readily make their way into the splenic vein as we have just seen the other constituent elements of that substance do.

The basis of Kolliker's main theory is, that extravasation of blood into the parenchyma of the spleen is of such constant occurrence, that it may be viewed as much a physiological, as a pathological phenomenon. That extravasations of blood may and do take place in the spleen, of course there can be no doubt; but we must confess, that we do not see what evidence there is for any such constant occurrence of extravasation, as that which Kölliker assumes. We cannot say that we have observed appearances in the red pulp of the spleen, which could be put down as unequivocal indications of extravasation having occurred during life. A piece cut out for examination from such a soft vascular parenchyma as that of the spleen, must necessarily be infiltrated with blood from the cut vessels.

The condition adduced by Professor Kölliker, on which the alleged extravasations depend, is "a kind of stagnation of blood, of course without stoppage of the circulation"! A stagnation of blood without stoppage of the circulation is rather a contradiction in terms; but if it be meant that there is congestion of blood-corpuscles within the vessels of the spleen, we can only say, that such a condition, though quite possible and likely enough, is not, and cannot be, an observed fact; and therefore is, in the present instance, of no more value than an hypothesis erected to support the other hypothesis, that extravasation of blood occurs so frequently in the spleen as to constitute a normal process.

But there are the blood-corpuscle-holding cells in the parenchyma of the spleen, at any rate, it may be said. This, however, is a point not so fully established. Many observers have failed to detect them; among others, Dr. Remak, of Berlin, who first drew attention to the subject, but who has since denied that blood-corpuscle-holding cells have any real existence, affirming that what has been taken for such are merely round clots of blood, and pigment cells. *And our own attempts to discover blood-corpuscle-holding cells have ended in a similar negative result.* To say nothing of our former investigation of the spleen, which was undertaken specially to ascertain what physiological relations exist between it and the corpuscles of the blood; in the course of which we never noticed anything like blood-corpuscle-holding cells, we have recently devoted several sittings exclusively to a search for blood-corpuscle-holding cells in the spleen. We have examined for this purpose three spleens of the frog (one frog was newly caught, one had been kept without food for a week, and the third for about three or four weeks), two spleens of the sheep, one spleen of a bat (Kölliker acknowledges never having found blood-corpuscle holding cells in the bat), two human spleens, and one rabbit's spleen.

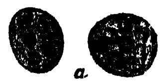
As the subject from which Professor Kölliker has taken his examples of blood-corpuscle-holding cells for delineation, we in particular subjected the spleen of the rabbit to a very searching investigation, but without discovering a single object which could be called a blood-corpuscle-holding cell.

We saw, indeed, numerous aggregations of red corpuscles, such as present themselves in a viscid plasma; many being rounded, and of the size ascribed to blood-corpuscle-holding cells. But, of course, we cannot, with Dr. Remak, suppose that Professor Kölliker could mistake such aggregations for 'blood-corpuscle-holding cells.'

We found numerous pigment granules, both free and enclosed; but nothing like a transition from *blood-corpuscle-holding* to *pigment-holding* cells.

Under these circumstances, it may be useful to examine somewhat closely the description which has been given of blood-corpuscle-holding cells.

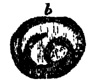
In the mammifera, it is said that blood-corpuscle-holding cells are not readily detected in the spleen, on account of the small size of the red corpuscles, and the readiness with which they give out their colouring matter. Small size of the red corpuscles! Surely in the present state of microscopy, there can be no difficulty on this score. Again: the blood-corpuscle-holding cells must be looked for without the addition of water to the piece of spleen under examination, as the water dissolves the cell-membrane, and bleaches the corpuscles. The cell-membrane must thus be different in chemical composition from that of ordinary cells. However this may be, we have succeeded as little without as with water. As to bleached corpuscles, an eye familiar with their appearance would have no difficulty in seeing such within or without a cell with a good microscope and good light. Kölliker's figures of blood-corpuscle-holding cells, from the spleen of the rabbit (here copied from page 267), might, for anything delineated to the contrary, be simply blood-corpuscles, embedded in small oval or round masses of lymphy substance.



a

We have above spoken of the occurrence of small aggregations of nucleated corpuscles in the pulp of the spleen, and in the blood of the splenic vein. Had those aggregations had a well-defined outline, and presented indications of a cell-wall, they might have corresponded with the above description. In connexion with this point, we may observe, that Professor Kölliker describes the nucleated fibres frequently met with coiled up, as contained within a cell, which, he says, becomes dissolved on the addition of water; but the annexed figure (copied from page 257) gives no indication of a cell-wall; and we never could determine the existence of anything more than a tenacious intercellular substance maintaining the fibre in its coil, and filling up the interior, and which we have distinctly observed in various degrees of laceration.

b



In the frog tribe, Dr. Kolliker says that the blood-corpuscle-holding cells are beautifully seen; and that, on account of the large size of the blood-corpuscles, the metamorphosis of the latter into pigment granules admits of being most distinctly traced. The existence of pigment cells in the spleen of the frog, as well as in the spleen of mammalia, is very certain; but we have not, as above stated, been more successful in discovering blood-corpuscle-holding cells in the spleen of the frog, than in that of mammalia: and in regard to the pigment cells, we have to remark, that their average size was only equal to that of the red blood-corpuscles of the animal; and therefore unfitted to be a receptacle for them; some, indeed, were larger, but many were much smaller. Moreover, it is to be observed, that the walls of these pigment cells were not so susceptible to

the action of water as those of the blood-corpuscle-holding cells are represented to be.

Though insisting so much on the existence of blood-corpuscle-holding cells, and their office of enclosing blood-corpuscles, preparatory to the breaking up of the latter, Prof. Kölliker still would have it understood, that he does not lay too much stress on the formation of a cell-wall around the little heaps of blood-corpuscles to be destroyed, as he finds that the blood-corpuscles may be broken up in the spleen, without their having been previously so enclosed in cells. And though he considers the spleen an organ in which blood-corpuscles are broken up, he admits that is not the only organ.

With these concessions, Dr. Kölliker virtually gives up the whole point of his theory of the function of the spleen; and we may conclude that *if blood-corpuscle-holding cells have a real existence* in that organ, it must be under unusual conditions, such as have never occurred to us; and that their physiological signification remains to be determined; for, assuredly, there are no grounds for admitting that attributed to them by Professor Kölliker.

T. Wharton Jones.

Review III.

Ueber Krisen und Kritische Tage. Von Dr. L. Traube.—*Berlin,* 1852.
On Crises and Critical Days. By Dr. L. Traube.

The views, which our most ancient authors, and principally *Hippocrates* and his commentator *Galen,* held on crises and critical days, have been frequently rejected in modern times as perfectly groundless; but since we have acknowledged again accurate observation of the process of nature to be one of the fundamental principles for the progress of medicine, we have learned that there is some truth in the old doctrine. A very interesting essay on this subject has been lately published by Dr. Traube of Berlin; it is the result of the careful observation of a large number of febrile cases, in which he had accurately fixed the commencement of the disease, and had marked twice on every day (during the time of remission and that of exacerbation) the temperature under the tongue, the pulse and other symptoms of importance. From the analysis of his cases he draws certain inferences which we shall proceed to quote and to remark on.

"I. *Fever consists essentially in an increased temperature of the blood.*"

It is scarcely necessary to remark here, that by the word '*fever*' the author does not mean a special disease, but only that complex of symptoms, which accompanies inflammatory diseases, acute exanthemata, &c., &c., and to which the Greeks very properly gave a distinct name πύρεξις, πυρετός, sometimes simply πῦρ, avoiding by this the confusion between '*fever*' as a real disease, and '*fever*' as a mere symptom or shadow of many different diseases.

The above-cited opinion of Dr. Traube is quite in accordance with the most ancient of all theories on fever, that of Hippocrates, Galen, Avicenna, and others—"Essentia vero febrium est—præter naturam caliditas" (ἡ παρὰ φύσιν θερμασία). Traube thinks that all the other symptoms accompanying the increase of temperature (fever-shivering, frequency of pulse, thirst, &c.) are caused by the abnormal heat of the blood, and he

promises to adduce the arguments for this view, in a future and longer work. For the present, however, we do not venture to look at one of the symptoms as producing all the others, but would consider the whole group of them, called '*fever*' (the increased temperature not less than the frequent pulse, the burning thirst, &c.), as the effect of a common cause, which we do not yet accurately know, but which we must look for in the reciprocal processes and changes taking place within and between the solid parts of the different organs and the *altered* blood in their capillaries, under the special influence of the nervous system.

"II. *The change from the abnormally increased to the normal temperature takes place either abruptly** (*within* 12—36 *hours*), *or gradually within a larger or smaller number of days.*"

An example of each kind of termination will clearly show the meaning of this inference. For the abrupt mode we may cite the following

"Case of pleuro-pneumonia in a man, aged 40 years; admitted during the exacerbation on the third day of the disease:

Day of Disease.	Exacerbation.		Remission.	
	Pulse.	Temperature.	Pulse.	Temperature.
III.	104	105°44	91	105°08
IV.	99	105°35	92	104°54
V.	97	105°08	80	101°66
VI.	72	99°95	56	98°78" (p. 6.)

For the gradual decrease we will cite,

"A mild case of typhoid fever in a weakly-framed female patient, aged 20 y.; the commencement of the disease on December 16th, at about 8 o'clock P.M., with intense shivering; admitted on the evening of December 26th—i.e., toward the end of the 10th day of the disease. The treatment was merely expectant.

Day of Disease.	Remission.		Exacerbation.	
	Pulse.	Temperature.	Pulse.	Temperature.
X.	..	..	112	105°08
XI.	100	102°74	108	103°64
XII.	100	101°48	104	104°54
XIII.	92	100°94	100	103°82
XIV.	92	100°4	100	104°18
XV.	84	100°22	100	103°82
XVI.	84	99°68	96	103°46
XVII.	80	99°14	88	102°74
XVIII.	88	98°78	80	102°2
XIX.	92	98°24	80	100°04
XX.	72	98°24	68	99°32
XXI.	76	98°645	68	99°50
XXII.	68	98°78	80	99°50"
XXIII.	80	98°42		(p. 6.)

* In the German original, 'sprungweise,' i.e. with a jump.

Several other cases are given for the explanation of § ii., the truth of which every one has opportunity of testing in daily practice. Pneumonia, acute cynanche tonsillaris, simple erysipelas, furnish frequently instances of the former, typhus and typhoid fever of the latter manner of termination.

We must, however, not understand Traube to assert that there does not exist another termination of acute diseases into health besides the two just described; in § viii. he speaks of a third kind as standing between the two, which he might have properly mentioned in this place, and which, in our as yet limited experience, has been found to be the most frequent one.

The abnormal heat becomes almost suddenly, within 12—36 hours, considerably diminished, but the temperature shows still an increase over the healthy standard of the individual, and this only gradually disappears during the subsequent decline of the disease.

Amongst 6 cases of pleuro-pneumonia and pneumonia, in which we have noted the temperature, in 2 only did it sink to the normal standard within 24—36 hours; in the 4 others it decreased considerably within a short space of time, but the remaining increase disappeared slowly. In one case, for instance, between the end of the 5th and that of the 6th day the temperature decreased from 105°5 to 98°5, then gradually during the following six days it fell to 97°0, the normal standard of that individual. To make this third manner of termination more evident, we shall give the outlines of one of these cases, which offers also some interest for other questions connected with the subject of crisis. The patient was a sugar-

Day of Disease.	Remission.			Exacerbation.			Urine.			REMARKS.
	Pulse.	Temp.	Resp.	Pulse.	Temp	Resp.	Quantity in 24 hours	Specific Gravity	Quantity of Lithic Acid in 24 hours.	Stool—Urine—Perspiration.
							Oz		Grains.	
III.	..	..	.	110	105°0	30				
IV.	105	104°5	34	112	104°5	28	46½	1·0195	12.2	One stool.
V	109	104°5	28	118	106°2	29	40	1·023	16 5	One stool.
VI	90	101°0	20	116	104°0	28	35	1 0205	16·5	No stool.
VII	90	105°0	30	108	106°4	31	12	1·018	..	Two fluid stools.
VIII.	108	105°5	28	124	106°4	.	10	1·0205	..	Two fluid stools; sedmt. of lithates
IX	110	105°5	26	120	106°0	28	31	1·0168	29·0	Much perspiration; no sediment
X	80	101°0	24	82	100°0	20	39½	1·0185	24·0	Much persp.; no stool, no sediment
XI	75	100°0	18	80	100°0	..	38	1·023	..	Two fluid stools; no sediment.
XII.	..	..	..	80	99°8	.	39	1·0195	22·5	Slight persp.; sediment of lithates.
XIII.	70	.	20	75	99°8	18	..	..	..	Two stools; no urine saved
XIV.	70	99°0	..	74	..	18	..	.	..	No urine saved; perspiration.
XV.	76	99°8	..	82	100°0	17	45	1·0175	16 5	One stool; sediment of lithates.
XVI	72	99°6	..	80	99°8	18	48	1·0165	18·0	Perspiration; no sediment.
XVII.	72	99°6	16	70	99°0	17	52	1·0165	..	Perspiration; no sediment.
XVIII.	68	..	..	..	..	..	42	1·023	..	Perspiration; large sediment.
XIX		98°8	..	68	99°0					
XX.	65	..	..	65	98°8	.	49	1·0185	14·5	Perspiration; sediment.
XXI.	62	..	..	68	98°8	..	40	1·024	..	No perspiration; no sediment.
XXII	62	96°6	16	65	97°6	16				
XXV.	62	98°2	..	66	98°6	..	40½	1·025	10·5	Sediment.
XXVI	62	..	..	62	97°8					
XXVII	60	98°2	15	64	98°2	.	37	1·0195	..	Perspiration; no sediment.
XXVIII.	60	97°8	15	62	98°2	16	..	..	7·2	Perspiration; no sediment.
XXIX.	58	97°8	..	60	97°8					
XXX.	..	..	..	58	97°8	..	38	1·0195	..	Slight perspiration.
XXXVI.	56	97°8	16		97°6					
XLII.	..	..	..	60	97°8	..	36½	1·016	7·0	

The urine was more or less acid as well during the whole of the disease as during the convalescence.

baker, aged 22 y., of phlegmatic temperament, well-developed muscular system, but of a rather hydræmic appearance (probably through his lowering occupation); three or four days after having been exposed to noxious influences he was seized with rigors on January 3rd in the morning, was admitted into the hospital on January 5th—i.e. in the first part of the third day of the disease.

On 6th day, in the morning, patient was much better in all symptoms; the dulness of percussion and bronchial respiration were confined to the lower part of the right side of the back, reaching upwards only to the middle of the scapula; on the 7th day, in the morning, the local phenomena had considerably extended as well upwards as towards the front; from the 9th day, gradually, the morbid phenomena disappeared, but on the 24th day, slight dulness with decreased vesicular respiration was still perceptible.—The treatment had consisted in small doses of antim.-pot. tartr. with nitr. of potash and opium in the beginning,—in hydrochlorate of ammonia (scrup. dim. quater die) after the 11th day of the disease.

"III. *The more rapid decrease of temperature is very often accompanied by considerable perspiration, less frequently by urinary sediments of lithates. In some cases both phenomena appear after the decrease of temperature, in others they do not appear at all.*"

Traube promises a fuller detail on these questions in his larger work. It is to be hoped, that he will then take also into account, not only the salts of the lithic acid, but also the lithic acid itself, which he does not at all mention in the present essay. From our own observation, which is, however, not yet sufficient to form a certain opinion, the excretion of the *lithic acid* appears closely connected with the process. In two cases of pneumonia and two of rheumatic fever the quantity excreted in 24 hours was found much increased during the days next to the critical change, as well before as after this, even to 29 grains, which is at least three times more than during the state of health (8 grains in 24 hours—*Becquerel*). In the one case of rheumatic fever and the one of pneumonia, in both of which the recovery was protracted and the temperature sank after a sudden considerable decrease only gradually to and under the normal standard, the quantity of lithic acid in the urine remained likewise abnormally great till the time of recovery. The numbers of the one case have been marked on the previous table. It may be also clearly seen from that table, that the appearance of the sediment of lithates in the urine does not depend on the quantity of lithic acid contained in the urine; on the 9th and 10th day 29·0 and 24·0 grs. of lithic acid were excreted in *only* 31 and 39½ ounces of urine, and in spite of this no sediment had appeared; on the 19th and 25th day it amounted only to 14·5 grains and to 10·5, which were contained in 49 and in 40½ ounces, and yet both times a large sediment of lithates had been formed soon after the urine was discharged.

"IV. *With the abrupt decrease of temperature, leading to recovery, a speedy and considerable diminution of the abnormal frequency of the pulse is almost always coincident.*"

To this inference Traube adds, that, on the other hand, sudden and considerable decrease of the pulse is *never* observed, but with coincident

remarkable sinking of the morbid heat, excepting in those cases which are under the influence of large doses of digitalis, and those affected with idiopathic disease of the brain and medulla oblongata. Although we have not yet observed a sudden decrease of the morbid frequency of the pulse, without coincident decrease of the abnormally augmented temperature; yet we think we ought to be cautious in stating such a coincidence as a rule without exceptions.

"V. *The sudden decrease of temperature may take place ere the process of inflammation, which was accompanied by the decrease of temperature, has ceased to spread.*"

As an instance of this, Traube gives a case of erysipelas faciei, in the decline of which the temperature considerably decreased on the beginning of the fifth day of the disease, although the local erysipelatous process had at the same time extended from the left to the right side of the face.

Without, as yet, contesting the assertion contained in this inference, we cannot help remarking, that we should have wished to see another case for its corroboration, as it is well known, that with the disappearance of the local phenomena in erysipelas, not only the temperature but also all the other constitutional morbid symptoms strikingly decrease.

"VI. *In protracted acute diseases, where the abnormal heat gradually disappears, towards the end the type of the fever becomes frequently that of the febris hectica, that is, the temperature is, during the period of remission, almost normal, or even abnormally low, but is considerably increased during that of the exacerbation of the same day.*"

The following table of a severe case of typhoid fever clearly shows the meaning of this paragraph:

Day of Disease.	Time of Remission.		Time of Exacerbation.	
	Pulse.	Temperature.	Pulse.	Temperature.
XIV.	108	105°08	104	104°81
XV.	100	103°28	100	105°62
XVI.	104	103°10	100	104°54
XVII.	101	104°0	104	104°67
XVIII.	104	103°82	104	104°90
XIX.	96	102°92	112	104°72
XX.	100	102°11	96	103°82
XXI.	92	103°86	104	104°54
XXII.	100	104°13	104	104°81
XXIII.	88	101°12	92	102°38
XXIV.	84	98°06	100	103°19
XXV.	84	98°37	92	102°83
XXVI.	76	98°51	108	104°36
XXVII.	80	99°14	96	104°90
XXVIII.	76	99°32	88	102°92
XXIX.	76	99°50	84	101°75
XXX.	68	96°755	76	101°03
XXXI.	76	98°37	84	101°03
XXXV.	84	99°41	76	98°19
XXXVII.	80	98°87	76	99°23

The same phenomenon we find well marked in one of two cases of *typhoid fever*, attended by Dr. Parkes, in University College Hospital, and in four out of six cases of the same disease attended by us in the German Hospital. The three of the eight cases in which the difference between the morning and evening temperature is not so striking, are all mild ones; in one of the five other cases, the typhoid fever was likewise mild, but the subject was debilitated by previous disease and starvation; in the four remaining cases the typhoid fever was severer, attended in one by frequent profuse perspiration and slight diarrhœa; in two others, by very copious diarrhœa during more than seven days; in the fourth, by considerable diarrhœa and loss of blood in the stools. The same phenomenon we observed lately in a case of peritonitis from a perforating ulcer of the small intestines, in which, during several weeks, almost no food was allowed through the mouth. From these and other cases we are inclined to look at this phenomenon as a consequence of starvation, or rather of the want of matter to effect and to undergo the warmth-creating processes, partly from the deficiency of nourishment taken and digested; partly from the accelerated loss of substance during the course of the disease: and we quite agree with Traube in considering it as an indication for support by diet and medicine, as far as the circumstances will permit it.

As these cases affirm also some other points of interest for our subject, we will shortly give the outlines of the two cases communicated to us by Dr. Parkes, the first of them showing a very considerable difference of the morning and evening temperature during several days; the second showing only a slight one.

Day of Observation.	Morning.			Mid-day.			Night.		
	Pulse.	Temp.	Resp.	Pulse.	Temp.	Resp	Pulse.	Temp.	Resp.
XIII.	...	...	...	104	101°5	...	100	101°0	
XIV.	100	...	40	98	97°0	28	108	102°5	*24
XV.	92	...	34	116	97°5	26	92	101°0	28
XVI.	98	99°0	30	85	94°0	24	108	97°0	24
XVII.	112	93°0	28	112	99°0	28	112	101°5	30
XVIII.	92	94°0	26	...	...	...	104	102°0	26
XIX.	96	100°0	36	84	98°6	27	104	102°0	24
XX.	96	96°0	28	96	98°0	24	106	103°0	28
XXI.	100	98°0	24	75	97°0	28	94	102°0	36
XXII.	90	96°5	28	84	98°5	28	116	...	32
XXIII.	84	96°5	20	80	96°0	24	110	101°5	30
XXIV.	76	96°0	20	74	97°5	22	104	101°0	30
XXV.	75	97°0	28	...	...	...	98	100°5	
XXVI.	90	98°5	28	84	98°5	28	98	98°5	

The patient was a woman, aged 25 years; considerable diarrhœa had been present from the time of admission into the hospital. During the night preceding the 17th day of the disease, a large quantity of blood was passed with the fluid stools; a smaller quantity during the course of the 17th day.

After the 18th day the diarrhœa ceased; after the 26th day of the disease, the difference between the morning and evening temperature was very slight; the convalescence appeared fully established, only a slight cough continuing for a few days longer.

Dr. Parkes' second case was a very mild one, in a male patient, aged 35. In the beginning there was some diarrhœa, well-marked rose-spots appeared between the 5th and 15th day; at first rather copiously, then sparingly; they had entirely disappeared on the 22nd, when the patient could be considered as convalescent.

Day of Observation	Morning.		Night.	
	Pulse.	Temperature.	Pulse.	Temperature.
V.	88	101°5	84	102°1
VI.	76	101°0	88	101°5
VII.	88	101°5		
IX.	98	99°5	...	100°5
X.	92	98°5	84	102°0
XI.	80	99°5	86	102°0
XII.	78	100°5		
XIII.	76	100°5	84	100°0
XIV.	74	99°0	80	99°5
XVI.	84	98°5	68	98°5
XVII	70	98°5	78	98°5
XIX.	..	98°5	60	97°5
XX.	80	98°5	...	97°5
XXII.	72	98°0	76	98°5

"VII. *If in acute diseases the abrupt sinking of temperature, leading to recovery, begins, as it generally does, within the first fortnight, then it is always either on the 3rd, or 5th, or 7th, or 9th, or 11th day, that this occurs.*"

Of the 52 cases analyzed, the temperature suddenly decreased in 30 cases; in 2 of these 30, the beginning of the disease could not be accurately fixed; in 1 the change took place on the 17th day; of the remaining 21, in 4 cases on the 3rd day, in 9 on the 5th day, in 11 on the 7th day, in 2 on the 9th day, and in 1 on the 11th.

Almost always, the sinking began within the 24 hours of an *odd* day, only 4 times between an *even* and *odd* day, never on an *even* day.

Traube thinks himself, according to inference IV., entitled to corroborate § vii. by 32 other cases of pleuro-pneumonia, in which he had not only noted the abrupt decrease of the frequency of the pulse, but that of the temperature.

It must be remarked here, that by the word '*day*' is not meant the period of 24 hours beginning after 12 o'clock at night, but (according to the example of Galen) "*a day of the disease*," i.e. a period of 24 hours, beginning with the appearance of the first symptoms of the disease. A patient, for instance, went to bed on the 19th of November in the feeling of health; he awoke on the morning of the 20th November, at 8 o'clock, with uneasiness, followed soon by fever-shivering;—the first day of the disease is considered in this case to commence, not at 12 o'clock in the night, but at 8 o'clock A.M. Every one will feel immediately how difficult, and frequently impossible, it is to ascertain at what time of the day the first symptoms of the disease made their appearance; and yet this is in-

dispensably necessary, if the observation is to be of any value for the proposed question. Amongst 62 acute cases, into which we lately accurately inquired with this purpose, in 18 only could we ascertain the commencement of the disease; in 5 of these cases the notes on the temperature are not sufficient to be analyzed for the present subject; of the remaining 12, the change took place rapidly in 8 cases, and of these—on the 5th day in 2 cases; between the 5th and 6th day in 1 case; on the 7th day in 2 cases; between the 7th and 8th day in 1 case; on the 9th day in 1 case; between 9th and 10th in one case.

The difficulty of accurately tracing the disease to its very commencement is still greater than in private practice, in those patients who can be made subjects of such examinations, as they are generally not accustomed to think much of their health, and to care for trifling uneasiness; and yet we all know that severe acute diseases frequently begin with a feeling of slight indisposition. And further, even if we know the hour when the first rigor occurred, are we always entitled to consider this as the outset of the disease? If we observe ourselves, or inquire from our friends, we frequently find that the fever-shivering, if there have been any, had been preceded for a greater number of hours by languor, uneasiness in the head, and other morbid symptoms. Quite in accordance with this is the observation of *Baerensprung*,* that the *temperature is already increased before the commencement of the rigor.* Once only we had the opportunity of examining almost immediately (about 8 or 12 minutes) after the beginning of a rather vehement shivering, in the case of a severe angina tonsillaris, which a female convalescent had contracted during her residence in the hospital. The temperature was 103°5 under the tongue, and 103°0 in the axilla, to which height we can scarcely suppose it had risen within 8 to 12 minutes; the normal temperature of that individual before and after the attack of angina was 96°—97°5. This statement of Traube's requires, therefore, additional and rigorous testing.

"VIII. *Not rarely during the decline of acute diseases, on the 5th, 7th, 9th, or 11th day, a sudden and remarkable sinking of temperature spontaneously takes place, which, though not leading immediately to recovery, is followed by a considerable and permanent decrease of fever. Never, as yet at least, have I met with such an occurrence on one of the intermediate* EVEN *days.*"

This would constitute a third manner of termination, to which we previously alluded. Five of the thirteen cases mentioned in the preceding paragraph belong to this head, but also of the eight others, four might be mentioned here, as after the abrupt sinking of the abnormal heat a small increase remained for several days longer. Of the five cases not analyzed in the preceding paragraph the change took place in one between the 5th and 6th day, in one on the 7th, in one between the 7th and 8th, in one on the 9th, in the fifth of the cases (erysipelas faciei et capitis) the decline was rather uncommon. The temperature decreased between the 7th and 8th days from 105°0 to 100°0, rose again on the 9th to 105°5, sank then on the 12th to 99°5, and in the following four days to 97°0. It appears to us, however, that the change taking place on the 12th day must not

* Müll. Archiv., 1852, pp. 217—286.

necessarily make us look at this case as at a proof against the inference of § viii., but the decrease after the 7th day may be considered as an incomplete crisis, the sudden increase on the 9th as a relapse, the crisis of which occurred on the 12th day—i. e., the 3rd day of the relapse.' The following outlines of the case will permit every one to form his own opinion.

The subject was a rather delicate young man, aged 22; fever-shivering in the afternoon of June 1st; admitted on June 5th during the morning—i. e., towards the end of the 4th day, when the local erysipelatous affection was limited to a part of the right side of the face, gradually spreading during the three following days over the right ear and right side of forehead; on the 8th day, local symptoms slightly diminished, constitutional almost disappeared; on the 9th day, and on the two succeeding, spreading of the local affection to the left side, which gradually disappeared after the 12th day. Treatment expectant with saline draughts (Sodæ sulph. ℥ss., Aq. font ℥ij.) on the 5th, 6th, 9th, and 10th days.

Day of Disease.	Time of Exacerbation. Pulse.	Time of Exacerbation. Temp.	Time of Remission. Pulse.	Time of Remission. Temp.	Urine. Quantity in 24 hours.	Urine. Spec. Grav.	Urine. Lithic Acid in 24 hours.	REMARKS.
					Oz.		Grs.	
V.	105	102°5	109	103°5	36	1·0185	..	One stool.
VI.	109	104°5	104	104°0	12	1·0215	..	Three fluid stools.
VII.	106	103°5	96	105°0	38	1·0205	26·5	No stool.
VIII.	85	100°0	84	99°5	44	1·019	24·5	Profuse persp.; sedmt. of lithates; 1 stool.
IX.	86	99°0	108	105°5	..	..	..	No urine saved; 1 stool.
X.	112	105°5	104	105°0	10	1·0225	..	Four fluid stools.
XI.	110	105°0	98	104°5	11½	1·024	..	Three fluid stools.
XII.	105	105°5	94	99°5	40	1·0205	29·5	No stool; perspiration.
XIII.	92	98°3	88	98°0	41	1·021	25·0	No stool; much persp.; slight lith. sedmt.
XIV.	90	98°0	..	.	.		..	No urine saved; 2 stools.
XV.	85	97°5	82	97°0	34	1·024	..	Two stools; much perspiration.
XVI.	80	97°4	76	97°0	39	1·018	..	Much perspiration.
XVIII.	76	97°0	74	97°2	42	1·0195	10·5	One stool; slight persp.; slight sediment.
XXI.	75	97°0						

"IX. *If during the decline of an acute inflammation attended by fever the increase of temperature is disappearing abruptly, and if it began to do so on one of the above-named days* (§ vii.), *the process of inflammation at the same time ceases to spread.*"

The best proof for this proposition may be found, according to the author, in cases of pneumonia confined to the upper lobe; after the sudden disappearance of the morbid heat, the dulness of percussion *never* increases in circumference or intensity; on the contrary, all the physical symptoms indicate a speedy resorption of the inflammatory exudation. On this point we have no evidence to offer.

"X. *The inflammation can, however, continue (though generally only for a short time) after the disappearance of the abnormal increase of temperature, in that part of the organ which was already previously affected, and in this continuance may remain a predisposing cause for a later spreading of the inflammation.*"

The author, in explanation of the first part of this paragraph, relates a case of *angina faucium*, in which the temperature sank during the 5th day from 103°28 to 99°32, and on the following day even under the normal mean of that individual, to 97°52, although in the local condition of the fauces, no change was observed before the second half of the 6th day, and, "even on the 10th day the inflammation had not yet entirely ceased."

"XI. *The abrupt disappearance of the increase of temperature (within 12–36 hours), in the decline of acute diseases, is in many instances not immediately succeeded by the normal degree of warmth, but by an abnormally low one, which only gradually passes into the normal state. The same is very often observed concerning the frequency of pulse.*"

This remark, as far as it regards the temperature, is in accordance with the experience of *Baerensprung* (l. c.), and we have had opportunity of corroborating it; more often, however, we have observed the decrease according to the manner described in § viii., and alluded to in the note to § ii.; in that case an abnormally low temperature frequently followed during the latter part of convalescence, and principally in weak people, or after a more protracted decline of the disease. Traube is much inclined to generalize the inference contained in § xi., partly induced by his own observation, partly by the view, based on *Chossat's* experiments on 'inanition,' that after the fever-exciting cause has disappeared, in consequence of the want of nutriment supplied during the fever, the normal medium of animal warmth cannot be produced; he considers, therefore, the temperature during the pyrexia as the product of two factors: the one tending to increase, the other to lower, the animal warmth, the former being the effect of the fever-exciting cause, the latter that of the insufficiency of food.

As to the frequency of the pulse, we have often observed it during recovery below the mean of the individual, but in several cases, where the temperature was too low, we found the pulse to be weak and more frequent than during health.

"XII. *There are probably two kinds of critical excretions:* (a) *such as form the cause of the sudden disappearance of fever,* (b) *such as are to be considered as the mere consequence of this disappearance.*"

Previously to examining the different excretions, Traube refers to the definition of '*crisis*.' The one given by Galen, in his third book on 'crisis,' is: "Crisis is called only the sudden change into health" (perhaps better, "towards health"). "This in general takes place through some manifest excretions or remarkable abscesses. And such like excretions and abscesses are preceded by an unusual ('not a little') perturbation in the body of the diseased." (*Μόνη τοίνυν ἁπλῶς κρίσις ἡ εἰς ὑγείαν ὀξύῤῥοπος μεταβολὴ προσαγορεύεται, καὶ γίνεται μὲν πάντως ἐπὶ φανεραῖς τισιν ἐκκρίσεσιν ἢ ἀξιολόγοις ἀποστάσεσιν. . . . Ἡγεῖται δὲ τῶν ἐκκρίσεών τε καὶ ἀποστάσεων τούτων οὐ σμικρὰ ταραχὴ κατὰ τοῦ καμόντος σῶμα.*) Although Traube highly values this definition, he has three objections against it. He does not admit the existence of a *crisis of the disease*, but only of the *fever* (pyrexia) accompanying the disease, as there does not occur a sudden disappearance of all the morbid phenomena, but only of the abnormal heat, by the decrease of which the principal condition for a speedy recovery is given.

In favour of Galen it can be said, however, that the words μεταβολὴ εἰς ὑγείαν might be interpreted as meaning the change into the way towards health; the Latin translation in the edition by Kühn (vol. ix. p. 703), saying, "ad sanitatem conversio."

Traube's second objection is, "that complete crisis may take place, without any remarkable pheromena in the different apparatuses of secretion."

In corroboration of this objection, he gives the outlines of a case, which we shall copy here, in order that every one may form his own opinion.

"Case of *angina faucium* in a strong girl, aged 21. The disease began in the morning with pain in the left submaxillary region, which was already on the following day so intense, that patient could not move the lower jaw. When admitted during the exacerbation of the 7th day the condition was as follows: The space to which the lower jaw can be removed from the upper one, measures scarcely ⅜ of an inch; tonsils much swollen and very painful; submaxillary lymphatic glands of left side enlarged and tender; constant flowing out of saliva, great difficulty in deglutition, not in respiration. Upwards to the 9th period of remission the treatment was merely expectant; only when at that time, in spite of the decrease of temperature, spreading of the inflammation from the lymphatic glands to the adjacent intercellular tissue had been observed, twenty leeches were applied to the left submaxillary region; internally also then no medicine was administered.

Day of Disease.	Exacerbation.		Remission.	
	Pulse.	Temperature.	Pulse.	Temperature.
VII.	...	...	100	101°12
VIII.	92	102°74	104	104°0
IX.	76	101°8	84	102°11
X.	64	99°64	68	100°04
XI.	64	99°05	64	99°68

"The abrupt decrease of temperature, which in this case took place on the 9th day, was neither accompanied by perspiration nor by urine disposed for the deposition of sediments of lithates: also after the decrease of temperature none of these phenomena occurred." (p. 21.)

In defence of the definition of *Galen*, we must remark that he does mention, not only manifest excretions, but also remarkable abscesses (ἐπὶ φανεραῖς τισιν ἐκκρίσεσιν ἢ ἀξιολόγοις ἀποστάσεσιν), and we are inclined to think, that he does not mean merely abscesses in the common sense of the word, but also visible depositions, eruptions, &c., which in some diseases accompany the change toward health; as, for instance, the appearance of the local phenomena (redness, swelling, bullæ, &c.,) in the erysipelatous process, of those in the process of herpes zoster, as that of an eruption on the skin in several other morbid processes, accompanied by pyrexia (exanthematic diseases), as also in some others, that of inflammatory infiltration of lymphatic glands and cellular tissue. Interpreting Hippocrates and Galen in this manner, we are inclined to suggest whether "the spreading of the inflammation to the adjacent cellular tissue" in the above given case was not a so-called critical deposition or an ἀξιολογος

ἀπόστασις. At any rate, we can scarcely consider ourselves entitled to conclude, merely from the absence of perspiration, or of sediments of lithates, &c., that during the period of change no critical excretion of any kind has taken place.

The last assertion is thus stated :

"Finally, I consider the perturbation in the nervous system preceding the crisis as being likewise unimportant, even more so than the critical excretions, as I have extremely seldom observed it." (p. 21.)

In favour of Galen we may say, however, that he does not speak merely of "a perturbation in the nervous system," but "of not a little perturbation in the body of the diseased" (οὐ σμικρὰ ταραχὴ κατὰ τοῦ καμόντος σῶμα), by which, according to our opinion, he may have meant the fever-heat, the frequency of pulse, the restlessness, &c.; and certainly every one of us has sufficient opportunity to remark such symptoms of perturbation of the system as frequently preceding in the stadium acmes of acute diseases the more or less sudden change towards health.

Traube's definition of crisis is the following :—

"*Crisis* is that termination of acute diseases attended by pyrexia, in which the change to convalescence is introduced by the sudden disappearance of the pyrexia (Fieber), i. e. of the abnormally high temperature. Opposite to this stands the *lysis*, in which the increase of temperature disappears under gradual continual decrease. As it were in the middle between the two, stands that manner of termination which is described in § viii., the abnormal heat sinking abruptly on one of the critical days, but not completely, so that a moderate augmentation of temperature remains for a few days longer."

We have remarked already, that we cannot yet look with Traube at fever and abnormally increased temperature as being identical; we think that he is quite right in not admitting the sudden change towards health to be induced *by the so-called critical excretions and abscesses*, but we cannot yet find any sufficient proof, that it is induced *by the abrupt sinking of the high temperature*. We must at present consider all the different phenomena of pyrexia to be equally dependent on the change in the morbid processes within the system, as yet so little known to us. True, it may be, that in these phenomena the change is earliest, most constantly and most easily observed in the temperature, but by accurate examination we shall find it also in the action of the heart, in the excretion through the lungs, the kidneys, and other organs, in the condition of the skin, &c.

Returning to Traube's classification of the so-called critical excretions, we find that he considers the *perspiration* and the *urine* disposed to the spontaneous formation of sediment as decidedly *after-critical* (nachkritisch), i. e., appearing *after* the crisis is already effected. The arguments for this inference are, that, 1, in cases terminated by a "*crisis completa*," he never saw the critical urine and perspiration *precede* the decrease of temperature; 2, he saw several times, in acute diseases, copious perspiration *without* being followed by decrease of temperature; 3, that artificially produced copious perspiration (by Priessnitz's method) *had no effect* on the temperature. In some diseases, however (principally in *rheumatismus articulorum acutus*), he admits that the perspiration can be "the cause of crisis, i. e., of the sudden decrease of temperature."

Traube attributes a really *critical* influence to *bleedings* and to *alvine*

evacuations. Concerning the former he has constantly found even small detractions of blood to be followed by considerable decrease of temperature. After three large venesections (amounting to 14, 16, and 20 ounces), we have likewise observed a sinking of from 0°8 to 2°0 Fah. (in cases of pleuro-pneumonia), but we have not been able to ascertain the same phenomenon after small local bleedings (4 to 8 ounces). Only in two cases out of seven we found a decrease of 0°6 and 1°0 Fah., but as this decrease coincided with the beginning of a gradual change towards health, we were not sure, whether to attribute it to the loss of blood or to the natural progress of the case. Also Von Baerensprung, who, in accordance with Nasse, observed a decrease after bleeding in dogs, has not noticed the same in men, except after considerable venesections. As a proof that spontaneous bleeding has the same effect, Traube gives a case of pleuro-pneumonia, in which an hæmorrhage from the lungs (amounting to about 6 ounces) occurring on the 4th day was followed by a remarkable decrease of temperature (1°8 Fah.), of the frequency of pulse and respiration, although the crisis did not take place before the 5th day. In a case of ulceration in the lower part of the small intestines in a young man, aged 22, a profuse hæmorrhage through the bowels (amounting to almost 2 pounds, without any diarrhœa), occurring on the 23rd day, was followed almost immediately by a rapid decrease of temperature (from 103° 0 to 91° 0 within two hours); after several hours' sleep the warmth had not returned to the amount noted on the night previous to the hæmorrhage, but almost to the normal standard, when a second smaller loss lowered it again considerably; the same sinking after new accesses of bleeding we remarked in this case three times, but in less than 18 hours after each access the temperature had increased again over the normal standard, except after the first one, which was so speedily followed by the second.

Also in Dr. Parkes' first case, previously given, we are inclined to attribute the very low degree of the morning temperature of the 17th and 18th day, to the loss of blood through the bowels; although this was accompanied by diarrhœa, we cannot consider the latter to be the only cause of the remarkable sinking of temperature, as it existed already on the previous days, without being attended by the same phenomenon in such a degree.

Traube ascribes a similar influence to the *alvine evacuations*, in case they occur shortly before or on the critical days.

The lowering power of profuse diarrhœa on the temperature we have likewise had an opportunity of observing, but we have not as yet been able to ascribe to it a critical influence. *Baerensprung's* (l. c.) researches show the same phenomenon in cholera, and not rarely we meet with it in delicate persons suffering occasionally from diarrhœa.

"XIII. *There are* REMEDIES, *by which the abnormally high temperature and the symptoms dependent on it can be considerably diminished; among the remedies, whose action I know by my own experience, I consider as such,* BLEEDING, DIGITALIS, CALOMEL *(in large doses), and* WATER, *provided its temperature be lower than that of the body. But never have I observed, that these remedies were able to induce a complete crisis on a non-critical day. Bleeding I have seen several times followed by a complete crisis, when*

it had been instituted shortly before or at the commencement of a critical day."

To show the influence of CALOMEL in large doses so as to produce copious evacuations, the author gives two cases of typhoid fever and one of pleuro-pneumonia. As the nature of the first case of typhoid fever might be doubted, we shall give the translation of the outlines, to allow every one to form his own opinion.

"Case of typhoid fever (Abdominal-typhus)* in a strongly-built man of twenty years. Beginning of the disease at 9 A.M., with rigors, followed by pain in the head and in the lumbar and sacral region. Admitted during the exacerbation of the seventh day (August 5th, 1850); an emetic had been administered on the fourth day."

Day of Disease.	TIME OF REMISSION.		TIME OF EXACERBATION.	
	Pulse.	Temperature.	Pulse.	Temperature.
VII.	...	...	100	104°90 [1]
VIII.	103	103°46	106	104°0 [2]
IX.	84	99°86 [3]	98	103°28 [4]
X.	76	101°30 [5]	84	101°66 [6]
XI.	76	99°86 [7]	88	102°38
XII.	76	100°22	78	100°94
XIII.	68	99°86		

[1] Soon after the time of this observation, a cold half bath (71° Fahr.). Remained in it during eight minutes and a half.
[2] At half-past 3 P.M. 5 grains of calomel had been administered, the same dose to be repeated three times in intervals of three hours.
[3] Between yesterday, 9 P.M., and now three copious greenish stools.
[4] Had another dose of 5 grains of calomel at half-past 11 A.M.; is to have the same once more at 6 P.M.
[5] Since yesterday, 9 P.M., two copious green stools.
[6] During the afternoon, two more.
[7] Since yesterday evening, again two such evacuations.

"There can be no doubt concerning the diagnosis, as the patient, who never had suffered from ague, had considerable enlargement of the spleen when admitted; besides this, he had been affected with epistaxis during the three days previous to his admission; and, in addition to this, intense cerebral affection was present." (p. 32.)

After having explained his opinion, that the cold half-bath of the 7th day had no influence on the change after the 8th day, and after having added, that a tablespoonful of castor oil had been administered on the 10th, on the 11th, and 13th days (to promote the action of the calomel), Traube continues:

* The disease, which is called *abdominal-typhus* by many of the German physicians, is the same affection which *Louis* describes as "*Fièvre Typhoïde*" (Paris, 1841), and *W. Jenner* distinguishes as "*typhoid fever*," from "*typhus fever*." ('On the Identity or Non-Identity of Typhoid and Typhus Fever.' London, 1850.) Although, in the present case, the symptoms described are not characteristic of "typhoid fever," yet the word "Abdominal-typhus" cannot be translated otherwise.

"From all this we learn, that *at least* those changes in the temperature and in the pulse, which took place between the evening of the eighth and the afternoon of the tenth day, are to be attributed to the calomel, and this the more certainly, as I have at that period, even in the mildest cases of typhoid fever, never observed such a remission of the fever (pyrexia) when the treatment had been expectant." (p. 33.)

Without doubting Traube's diagnosis, we may remark, that we have never attained such a striking effect from calomel in large doses, although we have not rarely administered it in a similar manner in cases of typhoid fever; the calomel produced the well-known green stools, but never quite so favourable a change as in this case.

In the second case of *typhoid fever*, the temperature on the 9th day, as well during exacerbation as remission, was 105°35. After several copious calomel stools between the 9th remission (Feb. 18th, morning) and 10th exacerbation (Feb. 18th, evening), the temperature at the latter period had decreased to 102°2; then it gradually rose again to 104°72, at the 12th exacerbation, after which, during the night, some more calomel stools ensued on the following morning—i.e., at the time of remission of the 12th day—by a new decrease to 101°61; in the evening of the same day, however, i. e., at the time of exacerbation of 13th day, the temperature was 104°32, in spite of another copious green stool.

The third case given in corroboration of § xiii. is one of *pleuro-pneumonia* in a strongly-built female patient.

At the exacerbation of the 7th day the temperature was 103°10; after two copious calomel stools (during the night) the temperature of the following (8th) remission was only 98°24, of the 8th exacerbation 98°60, then increasing again to 100°04 at the 9th remission and at the 10th exacerbation.

It can be scarcely doubted, that in this case the remedy had produced the decrease of the temperature; but it may be asked, whether this lowering influence was a specific effect of the calomel, or merely that of the copious alvine evacuations; or, in other words, whether another remedy, say magnesia sulphas in a slightly purging dose, would have produced the same effect or not? The circumstance that profuse spontaneous diarrhœa is not rarely attended by diminution of temperature must make us suspicious. We have not personal experience enough to form a decided opinion; two cases which we lately met with make us inclined to think, that *saline aperients*, in purging doses, exercise, under some conditions at least, a lowering influence on the temperature.

Of the one case we will give a short outline:

A strong-looking young man, aged 25, was attacked on September 7th, towards midnight, with headache, rigors, &c.; on the 11th September, in the morning, he was admitted into the hospital suffering from *angina faucium* with intense pyrexia. The treatment was merely dietetic, except that the patient took two ounces of *sodæ sulphas* in eight ounces of water, between the end of the 4th and that of the 5th day of disease (9 P. M. of Sept. 11th to 9 P. M. Sept. 12th), which was followed in the night from Sept. 12th to Sept. 13th by 4, and during the course of the 13th Sept. by 5 copious stools; after Sept. 13th the alvine evacuations became rare.

Day of Disease.	Time of Remission.		Time of Exacerbation.	
	Pulse.	Temperature.	Pulse.	Temperature.
IV.	95	103°5	101	105°2
V.	98	104°0	98	105°4
VI.	92	99°5	88	99°0
VII.	90	100°5	92	103°0
VIII.	85	101°5	90	102°0
IX.	80	98°0	70	97°5
X.	62	96°4	60	96°8
XI.	58	97°0		

We may be almost certain in this case that the decrease on the 6th day was effected by the remedy, the more so as the temperature rose again soon after the relaxation of the bowels had ceased.

Similar, though not quite so striking, was the effect in another patient, suffering likewise from *angina faucium* complicated by *bronchitis*, to whom two ounces of *magnesiæ sulphas* were administered between the end of the 3rd and the first half of the 4th day. The temperature at—

10 A.M. on 4th day was 104°5 (pulse 105).
6 P.M. „ 101°5 (after four fluid stools).
9 A.M. on 5th day was 98°0 (pulse 58, irregular, after seven fluid stools).
5½ P.M. „ 100°0 (pulse 95, after one stool more).
9 A.M. on 6th day was 104°0
6 P.M. „ 103°5

After the 8th day the temperature gradually decreased.

We must add, however, that in a case of acute rheumatism, attended by a high degree of pyrexia, we have seen no decrease of heat after two ounces of magnesiæ sulphas, administered during the second half of the 6th day, although the consequent relaxation of the bowels was considerable on the 7th and first part of 8th day.

Concerning the important question, whether there is any difference in the power of the remedies to effect a complete crisis according to their being administered immediately before and on an *odd* day or on an *even* day, we are not yet able to give an opinion either in favour of or against Traube's assertion.

"XIV. *By the frequency of the pulse we often cannot judge of the intensity of the fever (pyrexia). Sometimes I have found the former abnormally increased with normal temperature, at other times normal or even abnormally low with increased temperature; and thirdly, I have often observed a considerable evening exacerbation of the temperature, when at the same time the frequency of the pulse was only so slightly increased, that the difference between this and the morning pulse was scarcely perceptible.*" (p. 35.)

Several of the tables of cases contained in the author's essay can serve as arguments for the truth of § xiv.; also Dr. Parkes' two cases of typhoid fever (note to § vi.) may be used for the elucidation of its correctness. So in the first of these cases, on the 17th day, in the morning, the temperature was as low as 93°0, at night 101°5, and yet the frequency of the pulse at both periods was the same (112); on the 21st day in the

morning, pulse 100, temperature 98°0; at night, pulse 94, temperature 102°0.

It is satisfactory to remark, that by this observation the view of our oldest observers is fully corroborated; as *Galen* clearly expresses his opinion, that "the essential nature of fever does not consist in the motion of the arteries . . . . but in the abnormally increased heat." It was *Boerhaave* who held the opinion that the frequency ('velocitas') of the pulse was the most essential symptom of fever, saying in his aphorisms, "Quæ quidem symptomata in omni febre adsunt (horripilatio, pulsus velox, calor vario febris tempore vario gradu), sed *sola velocitas pulsus* adest ex his omni febris tempore, ab initio ad finem, eâque solâ medicus præsentem febrem judicat." *Boerhaave* certainly was not right in this opinion, neither was he correct in adducing the "stadium frigoris" of ague as an argument that fever may exist without increased temperature, as the observations of *De Haen*, *Gavarret*, *Nasse*, *Schmitz*, and *Baerensprung*, sufficiently prove, that *also during the 'stadium frigoris' the temperature is much increased.* But we must also be careful in adopting *Traube's* aphorism: "*Solus calor* adest ex his omni febris tempore, ab initio ad finem, eoque solo medicus præsentem febrem judicat." Circumstances may occur during the course of any disease attended by fever, in consequence of which the temperature may be for a shorter or longer period of time much lowered. After hæmorrhage I have repeatedly observed the temperature during several hours below the normal medium of health, and of this Dr. Parkes' case is also a proof; we can scarcely agree with the view, that there was no fever at all during those hours. On that account we can also not adopt without modification *Traube's* first inference, that "fever essentially consists in an increased temperature of the blood." From our limited knowledge we might venture perhaps to express our view concerning the connexion between *fever* and *increased temperature* in the following manner:

The animal temperature is in its largest amount the product of the chemical metamorphosis of parts of the blood and organic tissue. This chemical metamorphosis being more or less accelerated during the course of fever, the temperature must be more or less increased. As the increase of warmth is almost proportional to the increase of decomposition (or better, of the chemical processes), and as the latter is to a great degree dependent on the intensity of the disease, the temperature may in *general* be looked at as a scale for this intensity; but it does not follow, that as long as the disease is intense, the temperature must be *always* increased; —no, the chemical change of matter is constantly under the special direction of the *nervous system*, and it may be almost suddenly increased or decreased through influences acting on the latter; instances for the lowering influence are given in the loss of blood through hæmorrhage, or in any power producing a state approaching to syncope.

If, however, any one considers *fever as equivalent to morbidly increased decomposition of substance during acute diseases,* then we quite agree with him in saying, that the degree of heat is a scale for the degree of fever.

In taking leave of Dr. Traube's essay, we highly recommend its careful perusal to every one who is interested in the science of medicine; we look forward with much pleasure to the promised more elaborate work of the able author on the subject of crisis. Certainly these observations may be of

the greatest importance, not only for the pathology, but also for the therapeutical management of acute diseases. Their investigation is not so easy as it might appear; careful and continued observations, according to a certain plan, are utterly indispensable. We shall mention here a few points which appear to us particularly requisite.

1. The exact commencement of the disease must be as accurately fixed as possible.

2. The temperature and the pulse should be registered at least twice on every day—i. e., at the period when both are in general the lowest, and at that when they are highest (period of *remission* and *exacerbation*). It must be borne in mind that the typical change in the temperature is not the same during the course of acute diseases and during health; while there is in the normal state a double sinking and rising within 24 hours, with the highest number at about 11 o'clock A.M., and 6 to 7 o'clock P.M., and lowest at about 4 A.M., and 2 P.M. ("Baerensprung in *Müller's Archiv.*" 1851, p. 126), we observe during the most acute diseases the lowest between 7 and 9 A.M. the highest between 5 and 7 P.M.*

3. The greatest accuracy is necessary in taking the temperature and frequency of the pulse; it is scarcely necessary to remark, that everything that has an influence on the latter must be excluded.

It would be advisable, not only to mark the degree to which the mercury rises, but also the time which elapsed before it reached the highest point; a comparison of different cases, measured by the same thermometer, will show interesting differences.

4. Besides the pulse and the temperature, the phenomena presented by the *urine* are to be registered—the quantity secreted in 24 hours, the specific gravity, the physical appearance, the acidity or alkalescence, with approximative degree of the one or the other, if possible the quantity of the uric acid, that of the urea, of the phosphates, the sulphates, &c.

5. The phenomena of the *skin* are to be noted under another head.

6. Those of the *stools* under another.

7. The outlines of the progress of the *physical* and *functional phenomena* might have a place on the same table.

8. The *therapeutical* and *dietetic* influences ought not to be forgotten.

9. The notes on these points are to be continued during several days, at least, after the decided commencement of the convalescence.

Only by carefully recording all these points, by comparing and analyzing a large number of them, can we venture to form conclusions on subjects so delicate as those treated of in the preceding essay.

In private practice the opportunity is not often given for executing such investigations; in hospitals we meet more frequently with cases which,

* It is scarcely necessary to remark that Ague forms an exception; immediately before the beginning of the paroxysms in ague the temperature begins to rise, increases rapidly during the stadium frigoris, until it reaches the height at the close of this stadium and at the very beginning of that of dry heat, remains then for a time unchanged, begins slowly to sink towards the commencement of the stadium of perspiration, during the course of which it decreases rapidly, showing, however, at its close, still an increase over the normal standard, which only gradually becomes diminished during the time of apyrexia, until it reaches its minimum shortly before the stadium frigoris of the following paroxysm.

Since this review was written, we have received a number of Schmidt's Jahrbuch, containing an abstract of an important paper on the normal temperature of the body, by Lichtenfels and Fröhlich. It will be found in the Chronicle at the end of this number.

when accurately observed, may gradually throw light on these obscure subjects; but, unfortunately, the number of medical men employed even at larger hospitals is far too small, and they are so overloaded with other duties that little time remains for the investigation of such matters, which, nevertheless, form the most necessary links in the great chain of phenomena through which we gradually may be led to a more thorough understanding of the laws of our organism—an understanding not less important for practice than for science.

Hermann Weber.

Review IV.

1. *Beiträge zur Kenntniss der Bright'schen Krankheit.* Von Dr. Reinhardt.

Contributions to the Science of Bright's Disease. By Dr. Reinhardt. (From the 'Annalen des Charité Krankenhauses.')—*Berlin*, 1850.

2. *Die Bright'sche Nierenkrankheit und deren Behandlung.* Von Dr. Frerichs.—*Braunschweig*, 1851.

Bright's Disease and its Treatment. By Dr. Frerichs.

3. *Zur Pathologie der Bright'schen Krankheit.* Von J. F. Mazonn. Erster Theil. Pathologisch-Anatomische Beobachtungen.—*Kiew*, 1851.

On the Pathology of Bright's Disease. By J. F. Mazonn. First Part. Anatomico-Pathological Observations.

4. *Ueber Uræmia und Uræmischen Krankheitscharakter.* Von Dr. Eisenmann.

On Uræmia and the Character of the Uræmic Disease. By Dr. Eisenmann. (From the 'Verhandlungen der Physikalisch-Medicinischen Gesellschaft in Würzburg, 1852.)

The paper by Reinhardt, which we have placed at the head of our list, is devoted to a consideration of the morbid anatomy and pathology of Bright's disease. Its author describes, with great particularity, the various morbid appearances which the kidneys present to the naked eye, and the minute structural changes which are revealed by a microscopical examination; he endeavours to trace the connexion between these various morbid conditions, and the result of his inquiries is the conclusion that the different outward appearances in the kidney which many pathologists agree in referring to distinct forms of renal degeneration, are, in fact, different stages of a single morbid process, which he calls a diffuse inflammation of the kidney. We propose now to examine the grounds of this opinion. The subject is not one of those which have a merely speculative interest for the scientific pathologist, but it is one respecting which it is of the highest importance that we should have definite and accurate notions. In the examination of this question we shall have frequent occasion to refer to the opinions of Frerichs, whose work on Bright's disease, now well known to British pathologists, has already received an extended notice in the pages of this Journal.*

The definition which Reinhardt gives of Bright's disease is sufficiently

* Vol. ix. p. 285.

precise and comprehensive. It is characterized, he says, during life by a definite series of symptoms, and especially by anasarca, which, however, is not constant; by an albuminous condition of the urine; and by certain striking changes in the kidneys observable after death, either a granular condition, or a considerable enlargement of the cortical substance. This definition will include all the various morbid conditions of the kidney which are described and depicted in the far-famed *Reports of Medical Cases*.

Reinhardt and Frerichs agree in describing what they consider three stages of morbid change in the kidney; and their divisions, though slightly different, agree in all essential points. Reinhardt's first stage he calls the *simple inflammatory stage*, that of Frerichs is *the stage of simple hyperæmia and of commencing exudation*. The second stage of Reinhardt is *the stage of fatty infiltration of the kidney*, that of Frerichs is *the stage of exudation and of commencing metamorphosis of the exuded material*. The third stage of both authors is *the stage of atrophy of the kidney*. The following, therefore, is the order of phenomena as interpreted by Reinhardt and Frerichs: an engorgement of the renal bloodvessels, an effusion of inflammatory products, a more or less complete and general metamorphosis of these products into fat, and finally atrophy and wasting of the kidney. The small contracted granular kidneys have once been fat; the large, pale, fat kidneys are in continual progress towards atrophy and contraction.

The first observation which we have to make, with reference to this systematized description of renal disease, is, that there is no proof whatever that hyperæmia or over-fulness of the bloodvessels, is either a cause or an antecedent of those exudations into the uriniferous tubes, which constitute an essential feature of the inflammatory forms of renal disease. We refer now to the rapid formation of epithelium within the convoluted tubes, and to the occasional replacement of the normal epithelium by puriform cells. These changes in the tubes, it is true, are accompanied by engorgement of the bloodvessels, and the phenomena occur almost, if not quite, simultaneously; but in the order of causation, the changes in the secreting cells stand first. The circulation is impeded in consequence of morbid changes primarily affecting the secreting cells, and retarding their functions. Engorgement of bloodvessels implies, not in increased afflux of blood, or a more rapid circulation, but a retarded and impeded circulation, the impediment being shown by the frequent occurrence of hæmorrhage from the Malpighian capillaries in the early stages of acute renal disease, and by tortuosity of the arteries, with great hypertrophy of their muscular coats, after long-continued morbid changes affecting the secreting cells.

The theory of the *oneness* of Bright's disease has apparently had its influence in leading our authors to overlook the importance of distinguishing the various kinds of exudation into the tubes, which occur during the inflammatory forms or stages of the disease. If these varieties of morbid products were appreciable only after the death of the patient, their distinction would have little practical value; a moderate amount of clinical observation, however, will show, first, that the precise nature of the pathological changes which are occurring in the kidney may, with few exceptions, be as readily detected by a microscopical and chemical examination of the urine during life, as by the most searching post-mortem inspection of the kidneys; and, secondly, that the various kinds of morbid products observed in the

urine have a widely different significance when viewed in relation to prognosis. With reference to this point, it is of the first importance to ascertain, in any case of recent acute albuminuria, whether the urine is clear and free from sediment, or whether it deposits morbid materials, and what is the nature of these materials—whether there are any forms of tube-casts, and what is their appearance—are they composed of blood or of pure fibrin? or do they entangle organic cells of any kind, and what is the nature of these cells? have they the character of renal gland-cells; and do they contain oil, or are they free from that material? or, lastly, do they more nearly resemble pus-corpuscles?

Reinhardt's paper is defective in the results of the clinical study of renal disease; his opinions appear to have been chiefly deduced from the examination of the dead body. No one who has directed his attention to the subject can have failed to perceive that in many cases a careful microscopical examination of the urine during the patient's lifetime affords important aid in determining the minute structural changes in the kidney, so that the investigation of the diseased organ is much facilitated by a previous examination of its morbid secretion. This observation may be illustrated by reference to certain appearances in the tube-casts during the inflammatory stages of renal disease. In a large proportion of cases some of the renal gland-cells are entangled in the fibrinous material of the casts, while in other instances the casts are transparent and homogeneous, apparently composed of pure fibrin, and do not entangle epithelial cells. Frerichs offers the following explanation of these different appearances:—"The fibrinous cylinders are covered with epithelial cells when the effusion occurs in those uriniferous tubes which yet retain their epithelial lining, while those casts which escape from tubes which have been previously deprived of their epithelium are homogeneous and transparent, and entangle only some nuclei or oil-globules." (p. 57.) This statement has an element of truth, but it involves an important error. Frerichs has taken no note of the diameter of these transparent wax-like casts. It will be found, however, that while some of them measure $\frac{1}{500}$ inch in diameter, there are others which do not exceed $\frac{1}{1000}$ inch. The larger casts have a remarkably sharp, well-defined outline; their average diameter is that of the uriniferous tubes, and they are formed in those convoluted tubes which have no epithelial lining, as may be readily shown by an examination of the cortical portion of the kidney. The small homogeneous wax-like casts have a less definite outline than the larger; and so far from indicating that the tubes in which they are formed have been deprived of their epithelial lining, their diameter, which corresponds with that of the free canal in the centre of the uriniferous tube, sufficiently indicates that they have been moulded within that space, and that the tubes from which they have escaped possess a complete lining of epithelium. This point we have verified by the repeated examinations of the kidneys after death, and by a comparison of their condition with the microscopic appearances which had been observed in the urine during the patient's lifetime.

Frerichs doubts the existence of any morbid process in the kidney, to which the term *desquamative nephritis* can be applied, and the same doubt has been expressed in this country. The appearance of renal epithelium in the urine is, Frerichs says, a result of the cells becoming entangled in

the fibrinous effusion which coagulates in the tubes, and escaping thence, drags away the epithelial lining. We are curious to know what explanation Frerichs and other supporters of this hypothesis would give of the not uncommon cases to which we have just now referred—cases in which there is an abundant serous and fibrinous effusion with very numerous tube-casts, and an entire absence of renal epithelium in the urine. In what way would they explain the modifications in the appearance of the cells which are cast off in other cases? And how would they account for the occasional replacement of epithelium by pus? What account is to be given of the undoubted fact, that many of the tubes after death are crowded with epithelial cells, which are at least *twice as numerous* as in a healthy tube? *How is this multiplication of renal epithelium to be explained, if not by a rapid formation and shedding of new cells?* The supposition that the appearance of desquamation in the tubes after death is the result of *post-mortem* decomposition of the tissues, is entirely inconsistent with the *positive* observation that the epithelium is present in abnormal amount. The cells have, in fact, increased in number, in the same way as can be observed on other mucous membranes, whose epithelium is rapidly formed, and as rapidly thrown off.

The stage of fatty degeneration.—In considering the subject of fatty degeneration of the kidney, it is very important to bear in mind that the morbid condition occurs in two distinct forms. The large *granular fat kidney*, which is represented in the third figure of Dr. Bright's third plate, is, in a large proportion of cases, a secondary condition, which has been preceded for a longer or shorter period by an inflammatory state of the organ. We have observed the approach of this form of disease under the following circumstances:

1. An acute attack of general dropsy, with scanty high-coloured, albuminous, and bloody urine, and an abundant desquamation of epithelium, is followed, after a period of three or four weeks, by an appearance of oil in some of the cells; and as the disease makes progress, the total amount of epithelium in the urine diminishes, while the proportion of the cells which contain oil is increased, until at length nearly all the cells are more or less distended with oil, many of the cells, as well as scattered oil-globules, being entangled in small transparent wax-like casts. After death the kidneys are found enlarged, the cortical substance pale, and having scattered through it the characteristic yellow granulations, which very much resemble the minute atheromatous spots which are often seen in the arteries. These granulations are found, on microscopical examination, to be composed of convoluted tubes distended with oil, which is partly free and partly contained in cells. In other tubes the epithelium appears opaque, but contains no oil, and the central canal is free from deposit, while in others again there is an accumulation of epithelium, or a fibrinous effusion, or both combined.

2. In other cases the approach of fatty degeneration is different. The disease is chronic from the commencement; the urine is highly albuminous, but frequently of the natural colour, and either free from sediment, or it deposits a light cloud, which contains some of the small transparent waxy casts before mentioned. After a period, which may vary from a few weeks to many months, these casts entangle oil partly in the form of

scattered globules, and partly contained in cells. The oily casts and cells continue until the fatal termination, and after death the kidneys present essentially the same appearances as in the cases last mentioned.

The second form of fatty degeneration of the kidney differs in many important particulars from the preceding. The kidney is enlarged, but the cortical substance wants the granulations which are characteristic of the first-mentioned form, and it has instead a mottled appearance. On a microscopical examination *all* the tubes of the cortical substance are found to contain an excessive quantity of oil, which is, for the most part, contained within their epithelial cells. This condition of the kidney is sometimes found both in the human subject and in the lower animals—in cats and in dogs—unconnected with albuminuria, or with other functional symptoms of renal disease;* while in other cases of dropsy, with albumen and oil in the urine, this mottled form of fatty degeneration has been the only anatomical change observable in the kidneys after death. It will be seen, therefore, that the two forms of fatty degeneration differ in these important particulars: 1st, that in the granular form of disease the fatty degeneration is partial, while in the mottled form it is general throughout the tubes of the cortical substance; 2nd, that in the first form of disease, albuminuria and what may be considered an inflammatory stage precede, sometimes for a considerable period, the signs of fatty degeneration, while in the second form a great degree of fatty degeneration may exist unassociated with an albuminous condition of the urine.

The stage of Atrophy.—We come now to the consideration of that condition of the kidney, the most remarkable outward feature of which is a diminution of size and weight. The atrophy affects primarily the cortical substance, the surface of the kidney usually becomes uneven and coarsely granular, and its vascularity is much diminished. The first and second figures in Dr. Bright's third plate are probably familiar to most of our readers.

We have already stated that Reinhardt and Frerichs agree in considering that these small granular kidneys have passed through the stage of fatty degeneration, and that atrophy of the gland is only a later stage of the same morbid process as that of which inflammatory effusion and fatty degeneration constitute the first and second stages. In this opinion Eisenmann and Mazonn also concur, although they differ from Reinhardt and Frerichs in respect to some points of less importance. Now, with reference to this question, we dissent entirely from the opinions of these pathologists. True it is that a careful and extended series of observations upon morbid urine and kidneys had led us to form a judgment upon the point in question before we had any knowledge of the opinions referred to, but we trust that we were not, on that account, less open to conviction by any evidence which might be adduced to prove the transition from fatty degeneration of the kidney to that contracted condition of the organ with which every pathologist is familiar. But as we have met with no such evidence in the course of our own study of morbid phenomena, so we find none in the writings to which we have access. None of our authors

* This form of fatty degeneration of the kidney is frequently found after death from diabetes. We may observe, that in the able review of Frerichs' work, before referred to, in this journal, the existence of a fatty kidney, *par excellence*, is admitted.

attempt to prove the transition in question by clinical observation of the urine, but their opinion appears to be based upon what they consider the various stages of morbid change in the kidneys, as determined by post-mortem inspection.

Our limits will permit us to indicate only some of the principal facts, which tend to prove, as we think incontestably, that the large granular fat kidney and the small contracted kidney are the result of two morbid processes as essentially diverse as is consistent with the fact of the two diseases affecting the same tissues.

The most characteristic feature of that form of disease which leads to atrophy of the kidney is a disintegration of the epithelial cells, which appear in the urine in the form of granular casts of the tubes. In consequence of this washing away of disintegrated epithelium from the tubes, the basement-membrane is left denuded, and subsequently the tubes, having lost their epithelial lining, either waste away entirely, or, as we believe, they may continue to secrete a serous liquid, and so become dilated into cysts. In consequence of the atrophy of the tubes, the meshes of the matrix, in which the tubes are packed, become narrowed, and the fibres appear relatively thicker. Frerichs describes a development of new fibrous tissue as an occasional occurrence, and Mazonn considers it to be a constant and a characteristic condition. As Frerichs doubts the very existence of the normal fibrous matrix, his evidence upon this point is of little value to those who believe in the existence of such a tissue.* Mazonn recognises the normal intertubular tissue, and believes that he can distinguish this from the newly-formed fibres which are the product of disease.

A very few of the tubes may usually be found to contain oily matter, and this occurs more frequently in the denuded tubes than in those which still retain their epithelial lining. The thickening, and finally the obliteration, of the Malpighian capillaries, and the hypertrophy of the arterial coats, occur in this as in all forms of chronic renal disease.

The points of contrast between the fat granular † kidney and the contracted granular kidney are chiefly in respect of the tubes, with their epithelial lining. In the contracted kidney the disintegrated epithelium is swept away in the form of granular casts, and the tubes thus left denuded either waste or grow into cysts. In the fat kidney, the epithelium, for the most part, retains its position, and undergoes fatty degeneration, the tubes neither become denuded nor waste, as in the contracted kidneys, nor do they, except in very rare instances, grow into cysts. The combination, in the same subject, of the characters of the fat and the contracted kidney, are so rare as to prove that, while the two forms of disease are not absolutely incompatible, they are by no means allied; indeed, their relation is rather one of antagonism, since, in the one case, the epithelium is disintegrated and swept away, while in the other it remains, and undergoes fatty transformation.

* We must confess that we are surprised at the doubts which have been expressed on this point. Kölliker has recently published a *Handbuch der Gewebelehre*, in which, at page 461, it will be seen that this accomplished microscopist has had no difficulty in finding the tissue in question.

† Some confusion has doubtless arisen from the application of the same term, 'granular,' to two essentially different appearances. The granulations in the first form of fat kidney are local accumulations of oil, but the firm projecting granulations on the surface of a contracted, or, as it is sometimes called, a *cirrhosed* kidney, are composed of atrophied tubes, thickened arteries and fibrous matrix, often blended with a hardened fibrinous effusion, but rarely with fat in any form.

That the fat kidney has no tendency to pass into the contracted one, is shown by the post-mortem examination of cases which have been for a long time under observation. Not long since we examined the kidney of a man who had been nearly four years ill. He first had dropsy in the autumn of 1848; the urine was highly albuminous, and contained oily casts and cells in November, 1849, the same in January, 1850, and again in December, 1851; after that we have no note of the urine. He died in March of the present year (1852). One kidney had been destroyed by a calculus in the ureter, the other was more than double the natural size and weight, and had all the characters of a granular fat kidney, but not a trace of denuded or atrophied tubes. In another case, which had been nine months under observation, the urine, from first to last, contained oily casts and cells. The kidneys were much enlarged, and presented all the characters of fatty degeneration, but not a trace of the process which leads to atrophy.

But the essential difference between the two forms of disease may be proved by evidence of another kind, which may appear more conclusive to some of our readers who, perhaps, have less confidence than ourselves in the results of microscopical observation. The chronic desquamative disease which causes the disintegration and destruction of the epithelium, and finally extreme wasting of the kidney, is in many cases one of the most insidious of maladies, and it may proceed to the extent of destroying a large portion of the epithelium of the kidney without the occurrence of dropsy or any other formidable symptom; when perhaps, suddenly, in consequence of some accidental cause, the most urgent symptoms of suppressed secretion arise, the patient soon dies, and the kidneys are found wasted, and many of their tubes denuded and atrophied. We have before us the notes of four such cases; one patient was suddenly seized with peritonitis, and died in a few hours, a second died with apoplectic symptoms, a third with delirium and epilepsy, and a fourth with obstinate vomiting, consequent upon suppression of urine. The first two patients were supposed to be in good health until the period of their sudden seizure. In the last case alone had there been any dropsical symptoms, and these were only very slight and transient. In all the cases the kidneys were in an advanced stage of that chronic form of disease which is characterized by denuded and atrophied tubes. In contrast with these cases, which are by no means rare,* we place the fact, that, according to our experience, the *granular form* of fat kidney *never* destroys life without the previous occurrence of dropsy, which is usually one of the most prominent and distressing symptoms. Again, we have very rarely met with a case of inflammatory disease of the kidney—such as Reinhardt and Frerichs agree in referring to the first stage of Bright's disease—unaccompanied with dropsy in some degree, and for a variable period. Now, according to the opinion of these pathologists, the kidneys of the four patients whose cases we have briefly mentioned, must have passed through an inflammatory stage, and a stage of fatty degeneration, before they finally arrived at the stage of atrophy; yet in three of the cases there had been no dropsy whatsoever, and in the fourth case a slight and transient œdema of the ankles—such as might occur in any debilitated subject—had formed the

* Besides the four cases referred to, others have occurred to us, and, if necessary, we could collect a numerous series. A similar observation has been made by Rapp—(Virchow's Archiv., vol. iv.)

only dropsical symptom. This supposition is so improbable that we should hesitate to admit the doctrine which is based upon it, even without what we consider the conclusive evidence of the minute structural changes in the kidneys. We therefore feel bound to dissent from the doctrine of the oneness of Bright's disease, as propounded by Reinhardt and Frerichs. The apparent simplicity of the doctrine is not in accordance with nature, and it therefore tends to cause confusion. While we recognise an inflammatory form of the disease, we must, for the purposes of accurate diagnosis and prognosis, distinguish between the various kinds of effused products, which are chiefly, besides serum and blood, epithelium, pus, and pure unorganized fibrin. We must distinguish the granular form of fat kidney, which may be a consequence of a previous inflammatory stage, from the mottled form of the disease, which is analogous to ordinary fatty degeneration of the liver, and not a consequence of inflammation. Finally, we must recognise the fact, that the small contracted kidney, although an occasional consequence of an acute inflammatory attack, is more commonly the result of a disease which is chronic from the commencement, and never, as we believe, a consequence or a later stage of either of the forms of fatty degeneration.

We purpose, on a future occasion, to discuss the important subject of uræmic poisoning, with especial reference to the hypothesis of the decomposition of urea, which has been recently put forth by Frerichs.

George Johnson.

Review V.

1. *The Practice of Surgery.* By James Miller, F.R.S.E., Professor of Surgery in the University of Edinburgh, &c. &c. Second Edition. —*Edinburgh,* 1852. 8vo, pp. 687.
2. *A System of Practical Surgery.* By William Fergusson, F.R.S., Professor of Surgery in King's College, London, &c. &c. Third Edition.—*London,* 1852. 8vo, pp. 846.

Most of, if not all, the readers of this journal, it may be presumed, are familiar with the previous editions of the works whose titles are above transcribed, and it is, therefore, scarcely necessary to make any apology for not examining their contents in detail. Indeed, such an examination would be almost impracticable, for a recapitulation of the subjects discussed in these volumes, with the most meagre commentary upon some of the more leading and prominent topics, would occupy greatly more space than could possibly be spared. The only convenient course that lies open is to institute a comparative examination of the manner in which some few important surgical questions are treated of by Mr. Fergusson and Mr. Miller; and in attempting to do so, it is not our intention to criticise, but rather to compare, the works of these two very able and eminent surgeons, and to endeavour to convey a fair idea, both of the point of view under which each writer regards his subject, and of the nature and extent of the information which, in conformity with their respective plans, it is their object to convey. With that view, passing over Mr. Miller's brief opening chapter on operations in general, we shall commence by comparing some of the doctrines set forth in the succeeding chapters on injuries of the head, with those contained in the corresponding portions of Mr. Fergusson's work.

Mr. Miller considers injuries of the head both more methodically and at greater length, than Mr. Fergusson. For example, Mr. Miller devotes a separate chapter, occupying seven pages, to "Injuries of the Scalp," while Mr. Fergusson, when considering fractures of the cranium, incidentally disposes of injuries of the scalp in the few following lines:—"If there be a wound in the integument, it must be treated as one would be in any other part; it may be *stitched*, strapped, or poulticed, according to circumstances." (p. 549.) The word "stitched" has been underlined in the foregoing extract, to contrast the practice it seems to authorize, with the rule laid down by Mr. Miller, that in scalp wounds "sutures are, if possible, to be avoided; experience having shown that here they are especially liable to prove the exciting cause of erysipelas." (p. 9.) Mr. Miller, however, does not object to tying arteries of the scalp; on the contrary, having premised that "simple incised wounds of the scalp are apt to prove troublesome by bleeding," he proceeds to say—

"The arterial point or points are to be exposed and secured by ligature. Pressure may, in some instances, succeed, but in general it is decidedly inferior to the use of ligature; being not only less certain as a hæmostatic, but also liable to induce sloughing, or at least troublesome ulceration in the compressed part." (p. 9.)

It is, we apprehend, commonly thought that ligature of an artery of the scalp is much better avoided, and not very frequently needed. A ligature on these vessels usually causes much pain, and, what is more important, is just as likely as a suture in the scalp to excite erysipelas. Moreover, there are few regions of the body where pressure can, in ordinary cases, be so easily and efficiently applied; though of course cases occur from time to time, in which it may fail, or be inapplicable. Neither Mr. Miller nor Mr. Fergusson notices the troublesome complication of hæmorrhage with erysipelas, or the difficult and embarrassing, but fortunately rare occurrence of obstinate bleeding from a wound of the temporal artery deep in the temporal fossa; nor is it necessary here to discuss the practice proposed in such cases by Mr. Mayo and by M. A. Berard.

Mr. Miller introduces his chapter on "Injuries of the cranium and their consequences," with an account of "concussion of the brain," extending over nearly ten pages. Mr. Fergusson devotes, altogether, to the same subject, about half a page, from which we extract the following passage respecting the treatment of concussion of the brain:

"At first the practitioner's object should be to rouse the patient from this condition; cordials, stimulants, warmth, and *bloodletting*, are the ordinary means; the latter plan is generally resorted to at the earliest period possible, and though, in so far as the mere state of the pulse goes, the method is in contradiction to the usual doctrine of not bleeding in a state of shock, I believe that in some instances it may be of the utmost value. The late Professor Reid, of St. Andrews, found that after producing concussion in a rabbit, the right auricle of the heart became unable to act in consequence of over-distension, and that the animal died under these circumstances; if, on the contrary, another was injured in a similar way, and a vein in the neck opened, to lessen the quantity of blood in the vessels leading to the heart, its action continued, and recovery took place." (p. 547.)

Mr. Miller shall serve as a commentator upon the preceding extract, and it were, indeed, superfluous to add a word to his excellent directions for the treatment of the first stage of concussion of the brain. The passage is a long

one, and does not, it is true, set forth anything new; yet its quotation is justified, both as it affords a fair specimen of Mr. Miller's matter and manner, and subserves our purpose of comparing the two works before us.

"*Treatment.*—This necessarily varies according to the severity of the injury and the intensity of its results; but more especially is it different at different periods of the case. In the first stage—that of depression—if we act at all, it will be with the view of favouring at least the commencement of reaction. An opposite procedure were plainly at variance with common sense; but, unfortunately, it is found to be not equally at variance with common practice. A man stunned by a blow or fall, and labouring under simple concussion, is often bled on the instant—or an attempt, at least, is made to bleed him—by the rash and thoughtless practitioner. In other words, a fresh and powerful agent of depression is exerted on the general circulation, when such depression is already great, and has probably brought life to the very verge of extinction. If blood flow from the wound in venesection, under such circumstances, perhaps life is lost; at all events, the direct untoward result of the injury is aggravated; and the case is rendered both more urgent and more protracted than it otherwise would have been. The lancet is certainly not to be used during this stage. In many cases we should be little more than passive spectators. The depression is not extreme, nor giving indications of long continuance; signs of reaction, on the contrary, are slowly manifesting themselves: and we await the natural progress of events. Not altogether idle, however. Although not engaged in active treatment, we are prepared for activity, when circumstances shall call for our interference. The patient is stripped and put to bed. His whole body is carefully examined. He cannot tell us whether or not other parts have been injured, besides the head. Besides an anxious investigation as to the existence or not of other internal injuries (Principles, p. 101), we must ourselves carefully examine each joint and bone; detecting fracture or dislocation, and having it immediately rectified, while circumstances are also peculiarly favourable for the required manipulations. (Principles, p. 732.) On recovering his senses, he has not to complain of a painful and distorted limb, now for the first time observed; but finds what was distorted duly replaced, and already some way advanced in the process of repair. The head is carefully shaved; and is placed on pillows, considerably elevated. If wound of the scalp exist, hæmorrhage is arrested, if need be; and approximation is effected in the ordinary way.

"Should the depression prove great and continued, plainly indicating risk to life by syncope, something more is required of the practitioner. He endeavours gently to originate reaction. Warmth is applied to the surface; and friction is used over the chest and abdomen. If this be not sufficient to turn the course of the symptoms, a stimulant enema of turpentine is given. If still the progress be downwards, an attempt is made to convey to the stomach some warm tea, or soup, or wine-and-water; and stimulants are held to the nostrils, for insufflation. These last, however, are always to be warily managed, so as to avoid risk of injury by their too free application to a patient at the time insensible of pain; and the giving of fluids by the mouth, too, must be effected with care, lest they pass into the air-passages, and produce asphyxia. So soon as reaction has begun, we cease from our auxiliary efforts; and again become passive onlookers; completion of the second stage being always safest in the hands of Nature.

"If stimulants are used at all internally, it must be only in urgent circumstances, and with much caution; begun with a sparing hand, and repeated warily. And in general, we are well content to do nothing in this way; knowing that moderate depression is a favourable occurrence; and that premature cessation of it, especially when followed by abrupt and marked reaction, is apt to prove most injurious. For, at first, we can never be certain that the case is one of pure concussion. There may be a lesion, by laceration, of the brain's substance. During

the existence of concussion's first stage, the case remains, practically, one of concussion still; circulation is weak in the torn part, as elsewhere; extravasation of blood does not take place from the open vessels; valuable opportunity is afforded for their closure by natural hæmostatics; and when at last—it may be after some hours—the natural reaction slowly sets in, and circulation is proportionately restored, still no escape of blood occurs; and the symptoms may remain those of mere concussion to the last. Whereas, had the period of depression been abridged, and reaction rendered not only premature, but also abrupt and active, circulation would have been restored in the injured part ere the open vessels had closed, blood would have been extravasated, and compression of the brain must have ensued. Or, even if no lesion of the brain have occurred, the case being in all respects one of mere concussion, still premature and excessive reaction is most hazardous; by tending not only to kindle an inflammatory process in the brain or its membranes, but also to secure its being of an aggravated and perhaps uncontrollable character.

"Thus, then, it is plain that two great errors may be committed in the treatment of the first stage of concussion. Blood may be drawn prematurely; lowering the vital powers still further; unnecessarily, untowardly, perhaps fatally. Or stimuli may be imprudently employed; too soon, and too freely; hurrying on reaction; and endangering life, either by compression in consequence of extravasation of blood, or by an inflammatory process of an urgent and untoward character. Let both errors be studiously avoided; for each is of a most grave nature. While we take care that the depression does not proceed too far, let us beware of doing anything to effect either a premature or an excessive reaction. And when we attempt to fulfil the former indication, let us beware both of inducing asphyxia, by the misconducting of ingesta; and of causing troublesome excoriation and subsequent inflammation in susceptible and important parts, by the spilling of irritant stimuli upon them." (pp. 16—18.)

Mr. Fergusson says nothing respecting the mechanism of concussion of the brain: Mr. Miller quotes from one of our predecessors,* and we presume adopts the account, there given, of M. Gama's experimental explanation of the manner in which concussion of the brain is produced: on that head we may be pardoned for observing that M. Nélaton and M. Desnonvilliers admit, that M. Gama's explanation is probably the correct one; but they both state, that they failed to obtain similar results on repeating M. Gama's experiments. It so happens, that the experiments in question were also repeated by the writer of this notice, and it may not be out of place to say, that he succeeded in distinctly reproducing the more essential phenomena described by M. Gama. Be the mode of production of concussion of the brain, however, what it may, the more interesting and important question remains, whether concussion is the consequence of, or is attended with, any appreciable physical alteration of the brain. Neither Mr. Miller nor Mr. Fergusson touch upon that point, and with so saying, any reference to it on our part should perhaps end, but as another opportunity for the purpose may not soon occur, we are unwilling to let the present one pass, without asserting the priority of a distinguished English physician, Dr. Bright, in having observed and described a condition of the brain after concussion, which there is much reason to conclude, though as yet the fact can scarcely be considered established, reveals the intimate nature of the injury sustained by that organ.

Two eminent French surgeons, M. Nélaton, in his 'Elements of Surgical Pathology,' and M. Desnonvilliers, in the 'Compendium of Surgery,' com-

* British and Foreign Medical Review, No. xx. p. 16.

menced conjointly with the late M. A. Berard, and, since his lamented death, continued along with M. Gosselin, have each given an account of certain morbid appearances observed after death from concussion of the brain. M. Nélaton, it will be observed, attributes to Sanson the merit of having first pointed out the appearances described in the following extract:

"Sanson, who had a predilection for studying every question relating to injuries of the head, was the first to indicate an alteration of the substance of the brain, which he drew our attention to several times. The alteration in question consists in minute extravasations of blood, as large as the heads of small pins, disseminated in the substance of the brain. Since our attention was directed to this fact, we have discovered, under similar circumstances, these small miliary extravasations, which might be confounded, were we not upon our guard, with minute drops of blood presenting at the orifices of the vessels when a slice of brain is cut away. But that mistake is easily avoided; for in the first case the minute clot of blood can be removed with the point of the knife, while in the second it is fluid blood that presents at the extremity of the vessel; and, moreover, it is possible to remove the little speck of blood and cause it to reappear several times in succession at the same point, by gently compressing the substance of the brain. It requires, no doubt, great care to always discover so slight a lesion; yet it will seldom escape observation, if we have the patience to first remove the pia mater and then pare away the brain in very thin slices. These extravasations of blood are found at the periphery and towards the centre of the brain; they occur equally at the point percussed and at points adjacent to it, and at others more or less remote. The small extravasations are sometimes numerous, sometimes few, and but five or six of them may be disseminated throughout the brain." (Nélaton, t. ii. p. 575.)

M. Nélaton then goes on to observe, that the alteration above described constantly exists in contusion of the brain also, and he thence infers that *concussion* must be regarded as a slight degree of *contusion*—an inference not warranted by the premises, as the appearance in question only shows, at the utmost, that bloodvessels are ruptured in both cases; and finally, M. Nélaton says, it by no means follows that the minute extravasations of blood above described are present in every case of concussion of the brain, because it is quite conceivable that a shock may suffice to rupture some of the cerebral fibres, without being sufficiently energetic to rupture the bloodvessels also, and the opportunity of examining the brain only occurs in fatal cases of the accident, in which the severer shock capable of rupturing the bloodvessels has been inflicted. The second volume of M. Nélaton's work was published in 1849, and we do not know any earlier notice of Sanson's having made the observation, respecting the condition of the brain in concussion, attributed to him by M. Nélaton. There is no allusion to the subject in the 'Nouveaux Elements de Pathologie Med. Chirurg.,' by Roche and Sanson, in which the latter writer expressly treats of injuries of the head.

In the 'Compendium of Surgery,' by MM. Desnonvilliers and Gosselin, two fatal cases of concussion of the brain, with the *post-mortem* appearances, are recorded. The first of these cases was communicated by M. Bayard to M. Desnonvilliers, and is peculiarly interesting, because it adds another to the extremely few cases in which any account is given of the condition of the brain, where death occurred very soon after the accident; and in which, consequently, the appearances cannot be referred to changes produced by consecutive complications. The circumstances, under which the

injury was inflicted, were almost identical with those in Littré's celebrated case. A man, aged twenty-three, was arrested in the suburbs of Paris, and having threatened to commit suicide, was confined in a cell with his hands tied behind his back; when left alone, he dashed his head violently against the wall, fell senseless, and died in about *three quarters of an hour*. On dissection, there was no fracture of the skull, no extravasation of blood, either between the dura mater and the bone, or on the surface or in the substance of the brain. Neither was there any contusion or laceration of that organ. The brain, moreover, presented its natural consistence; but throughout its substance were interspersed small specks of blood, and, on gently compressing the sliced brain between the fingers, fluid blood exuded from the vessels. In the second case death occurred *eight hours* after a fall from a considerable height. Several complications, however, existed in this case. There was fracture of the cranium with depression, and slight laceration of the brain at one point, with slight contusion of the organ at the point diametrically opposite, but no blood was extravasated, either on the surface or in the substance of the brain. On slicing the brain, however, appearances similar to those seen in the preceding case were discovered.

M. Desnonvilliers admits the accuracy of the account given by Littré of the condition of the brain, in the well-known case recorded by that writer; and he thence concludes, that when concussion of the brain is immediately fatal, the only cognisable alteration is a contraction or shrinking of the organ; with that point, however, we are not at present concerned. But M. Desnonvilliers further infers, from the two cases of which an outline has been given above, that when the patient lives for some time, the anatomical characters of concussion consist in congestion of the vessels of the brain. And when the patient survives the affection, he thinks it may be fairly assumed that the organic changes are the same in kind but less in degree.

We need not stop to point out the differences between the anatomical characters described by M. Nélaton, and by M. Desnonvilliers; but it will be seen that Dr. Bright's account of the appearances observed after concussion of the brain, embodies those that are given by both the French writers; one of whom he anticipated by eighteen, and the other by twenty years.

We have not space to quote as fully from Dr. Bright as we could desire; but it is, perhaps, enough to say, that in his *Reports of Medical Cases*, &c., vol. ii. part 1, 1831, Dr. Bright describes at page 408 the "small clots from laceration . . . . of the size of large pin-heads," occupying "the internal part of the brain," &c. and that at page 410 he writes as follows:

"What then is the immediate state of the brain producing the symptoms of concussion? Of this we can only judge from the nature of the injury traceable after death. . . . . and almost the only appearances which can be considered peculiar are the minute lacerations of the brain and vessels, which occur both upon the surface and deep in the substance of the brain; and we are led to conclude that the violence done to the brain, if it does not always go the length of producing these appearances, has at least such a tendency, and that it is this rupture of the brain, or an approach to it, with some consequent congestion in the vessels, which gives rise to the peculiar symptoms of concussion."

And a little further on Dr. Bright comes to the conclusion, which harmonizes so well with practical experience, that "a process of repair"—a "mode of healing,"—is required for the cure of concussion, probably "analogous to the repair of apoplectic injuries, requiring therefore much time, and calling for great caution in the management of the invalid for a long period after the first symptoms."

The condition of the brain in concussion requires, no doubt, further investigation; but the existing evidence, so far as it goes, strongly tends to show the connexion between concussion of the brain and the morbid changes above mentioned; and if further researches establish that connexion more completely, the merit of having first elucidated one of the most obscure and debated points in surgical pathology, will certainly belong to Dr. Bright. It is scarcely necessary to remark how much more satisfactorily all the phenomena of concussion can be understood by tracing them to the physical alterations indicated by Dr. Bright, than by the vague expedient of referring them to "functional derangement." It will probably, also, be admitted, that M. Gama's experimental illustration of the mode of production of concussion of the brain, quite accords with the appearances that have been observed in fatal cases. But this digression has made a formidable inroad on our space, and it is time to return to Mr. Miller and Mr. Fergusson.

Mr. Miller thinks the operation of trephining is generally inapplicable "in the case of compression by extravasated blood," because it is uncertain whether we shall hit upon the site of the extravasated blood, and if we should succeed in doing so, the blood may lie beneath the membranes of the brain; and, moreover, we may be unable to remove the extravasation even if we have exposed it; while, on the other hand, we are tolerably certain to greatly increase the danger of inflammation by the operation. To this general rule Mr. Miller admits the following exceptions:

"The exception consists in those cases of injury applied in the course of the middle meningeal artery, immediately followed by urgent symptoms of compression, with or without fracture of the skull, in which we can have little doubt of the following circumstances:—1. That the compression is caused by extravasation of blood; 2. That the blood has been extravasated at or near the point struck; 3. That the extravasation is situate exteriorly to the dura mater; 4. That the blood is yet mainly fluid, and therefore likely to escape readily outwards, on an aperture of communication being established; 5. That even if it have coagulated, extrusion may yet be effected, without necessarily exciting inflammation, either in the brain or in any of its membranes. Under such circumstances, we need not hesitate to apply a trephine to the injured part—when the symptoms of compression are sufficiently urgent to demand direct interference—with the full hope of affording most important and salutary relief.

"We can also conceive it possible, that an injury may be sustained at a part of the cranium not connected with the course of the meningeal artery; that the symptoms of compression by extravasation may be both very urgent and very plain; and that the surgeon, after careful examination and consideration of the case, may feel satisfied that the site of extravasation corresponds to the part struck. The trephine is applied. If blood be found at that part, exterior to the dura mater, the issue is most fortunate. But if no blood be found, two questions naturally arise: Are the membranes of the brain to be perforated? or is another part of the cranial contents to be exposed by reapplication of the trephine? The latter question is certainly to be answered in the negative; the former, in the

affirmative, only when the dura mater is elevated through the trephine-hole, tense, comparatively non-pulsating, perhaps fluctuating, or otherwise affording tolerably distinct evidence of the sought-for blood being lodged beneath." (p. 31.)

With reference to trephining in "compression by accumulation of pus between the cranium and the dura mater," Mr. Miller lays down the following rules of practice :

"If, on removing a portion of skull by the trephine, matter is not found, a question arises whether our efforts at direct relief are to cease, or whether further exploration is to be attempted. Is the dura mater to be perforated, in the hope that the site of abscess may prove to be beneath? Not, if the membrane present its usual normal characters at the part exposed; level, moving synchronously with the cerebral mass, smooth, of a brownish hue, and showing something of a silvery lustre. But if it be protruding through the cranial aperture, flocculent, non-pulsating, and either too dark or too pale in colour—and, more especially, if it afford anything of a feeling of fluctuation when touched—we need not hesitate to puncture, and need not doubt to find an issue of purulent or other fluid from the wound. If the dura mater appear sound, and its puncture consequently be unwarrantable, are we permitted to re-apply the trephine; either at the site of *contrecoup*, or in the immediate vicinity of the first application? Either of these procedures may be warrantable, if the symptoms of dura-matral abscess are peculiarly marked, and the surgeon is thoroughly convinced of its existence. But, as can readily be understood, the latter site of the reapplication is preferable. And, as already stated, a large size of trephine should be employed at first, to anticipate the necessity of such repetition. Only in very extreme cases should the site of *contrecoup* be trephined. Having failed in the indicated spot, we proceed to other explorations with great uncertainty. Fortunately, however, it is comparatively seldom that the site of abscess is elsewhere than at the injured part." (p. 33.)

But what are "the peculiarly marked symptoms," in virtue of which the surgeon is to be "thoroughly convinced" of the existence of "*dura-matral* abscess"? The most significant indications are the local, coupled with the general symptoms, so very well described by Mr. Miller, but which it is unnecessary to quote. But these indications are not conclusive. If they were, there would be no need of the precept to search for pus at the site of *contrecoup*, or elsewhere, after having failed to discover it at the point where the local, but fallacious, signs existed. And again, how is it to be known that there was any counter-stroke? or if one did occur, how is its exact site to be determined? for the counter-stroke does not uniformly take place at the point diametrically opposite to that which received the blow; and, very often indeed, when a counter-stroke has occurred, it corresponds to some part of the base of the skull.

Mr. Fergusson is much less reserved than Mr. Miller, in the use of the trephine in compression by extravasation of blood, or by pus; and, probably for that very reason, he is much less precise in specifying the conditions under which its employment is indicated. Mr. Fergusson draws no very clear distinction between compression by extravasated blood, and compression by pus—that is to say, so far as his rules of practice are concerned. He thus briefly disposes of the two questions :

"If a person had received a blow on the temple, and if the symptoms indicated effusion of blood from the meningeal artery, I should not hesitate to perforate the cranium over the supposed collection; and in the event of compression coming on in a number of days, and when suppuration might be suspected, the same method might be adopted. Here, however, it might be difficult to determine the part on

which the trephine should be applied. Where blood has been suspected, I have seen great hesitation on the latter point, and so also there may be in the supposition of an abscess being present. In the latter case, the wound on the surface would probably be selected; more especially if the bone around seemed diminished in its vascularity, or altogether deprived of circulation. [Here intervenes the description of the manner of performing the operation of trephining.] In the instance of fracture the operator can scarcely go wrong as to the proper part for applying the instrument; but in other cases he may discover that he has erred in his diagnosis, or he may then (not finding that which he has expected) think of opening another part of the cranium, either immediately contiguous or at some distance. Such proceedings are now rarely ever heard of; yet, under the desperate circumstances of the patient, I should consider the surgeon justified in making further search: provided always, that he has some degree of reason on his side (such as that the patient is not actually *in articulo*, for I have heard of a zealous practitioner persisting in the use of the instrument until it was hinted that his patient was dead); and there is no lack of precedent for such a course. Two, three, and four pieces—even a larger number—have often been removed with success; and Vander Wiel relates one instance, where the trepan was applied twenty-four different times with success." (pp. 549—551.)

Reaction is liable to occur in the practice of surgery as in everything else, and mediæval tendencies are now-a-days pretty strong. Modern surgeons very properly deviate widely from the over-cautious maxims of Desault and his school, yet we scarcely anticipate a revival of the heroic performances of the age of old Stalpartius Vander Wiel.

There is no material difference in the precepts laid down by Mr. Miller and Mr. Fergusson for trephining in *fractures* of the cranium.

Mr. Miller notices dislocations of the cervical vertebræ with great brevity; Mr. Fergusson considers them at greater length, but mixes up dislocations caused by external violence with those consequent upon disease of the spine. With reference to the treatment of traumatic dislocations of the cervical vertebræ, Mr. Miller says:—"If life, or the hope of life, remain, replacement is to be effected by careful extension and coaptation." (p. 302.)

Mr. Fergusson on this subject writes as follows:

"The cervical vertebræ are occasionally displaced, either as the result of accident or disease. When the former happens, it is customary to suppose that the spinal marrow is so affected that immediate death is the result, especially if the injury is above the origin of the phrenic nerve. I have not myself seen any case of this kind, but I believe that there are few in the profession who doubt the truth of the current doctrine: it has been asserted, however, that such displacements may be treated like luxations in the extremities—that is, by extension and counter-extension, and cases of the kind have been recorded in modern journals. The practice which has been proposed in such instances is not so novel as some people imagine. Heïster states that 'what is vulgarly called a broken neck is generally no more than a luxation, though sometimes the vertebræ are fractured. If life should remain after such a luxation, which very rarely happens, the patient's head is commonly distorted, with his chin close down to his breast, so that he can neither swallow anything nor speak, nor even move any part that is below his neck; therefore, if speedy assistance be not had, death ensues from the compressure or hurt of the medulla. But to repulse this unwelcome messenger, the patient is to be immediately laid flat upon the ground or floor; then the surgeon kneeling down, with his knees against the patient's shoulder, is to bring them together so as to contain the patient's neck between them; this done, he quickly lays hold of the

patient's head with both his hands, and strongly pulling or extending it, he gently moves it from one side to the other, till he finds, by a noise, the natural position of the neck, and the remission of the symptoms, that the dislocation is properly reduced."

"That the neck may be straightened in this way I believe, but I should imagine that the injury to the spinal marrow would be such that this 'unwelcome visiter' would keep his place. Heïster is certainly a very different authority on professional matters than Rabelais, yet by the above account one is forcibly reminded of the exploit of Panurge in replacing the severed head of his friend Episthemon." (pp. 540, 541.)

Heïster's account of the matter, no doubt, besides being somewhat grotesque, is quite too vague to satisfy the exigencies of modern science; but the cases "recorded in modern journals," and in some modern books also, deserve a more serious examination. We cannot pass those cases in review here, but we may say generally that they establish—*First*, what Heïster affirmed (though he completely reversed the relative frequency of dislocation and fracture), and what was subsequently long denied, that simple dislocation of the inferior cervical vertebræ may occur; *secondly*, that "immediate death" is by no means the necessary result of such dislocations; and, *thirdly*, that these dislocations have been reduced, and that the patients have recovered. On this latter point anatomical evidence is, of course, wanting, the patients having survived; but the cases recorded by Leveille (Desault's case), Rust, Newmann, Walther, Schuh, Willefeld, J. Guerin, Mignonneau, and Drs. Harrison and Ellis, leave no reasonable doubt upon that score.

Heïster made another proposal, respecting which Mr. Miller and Mr. Fergusson do not entirely agree—we mean, trephining in fracture of the spine; for Heïster, and subsequently Viguerie, suggested that operation, which was first performed by Mr. H. Cline. Mr. Miller thus unreservedly rejects the proceeding:

"In the obviously displaced spinal fracture, with symptoms of compression of the cord, it has been proposed to employ the trephine, with the view of relieving the injured medullary matter. Reason and experience, however, have decided against the procedure; inquiry having shown that the compressing agent is usually the fore part of the body of the vertebra, which cannot be reached and dealt with from without." (p. 301.)

Mr. Fergusson, in the following judicious observations, is less absolute in condemning the operation:

"The analogy between such injuries [of the spine] and those of the head, when there is depression of bone, though in some respects close, is widely different in others; thus, there may be extensive depression on one side of the cranium without any such condition on the other; but in the spine, when the column is broken, the irregularity will probably be as much on one side of the canal as on the other, consequently, pressure on the spinal cord may just be as great and as destructive in front as behind. However, it is possible to imagine an instance where a lamina is driven in without the body of the vertebra being affected; and, as the case may be in any way almost hopeless, it might be deemed advisable to give the patient the advantage of this poor chance." (p. 714.)

In fractures of the spine, Mr. Miller recommends "careful reduction of the displacement" (p. 301); and Mr. Fergusson does not object to making an attempt at reduction, though he questions its utility. "No harm, in my opinion," he says, "can arise from a moderate attempt at

extension, although its ultimate utility may be doubted; but the idea of forcing the protrusion into a proper position seems preposterous." (p. 714.)

The admissibility and value of the practice in question have certainly yet to be determined by future experience; but Mr. Crawford's case, referred to by Mr. Fergusson, and those published by Mr. Tuson, and by M. Malgaigne, are calculated to encourage surgeons to give it a more extended trial. Neither Mr. Miller nor Mr. Fergusson mention the use of Dr. Arnott's hydrostatic bed in the treatment of fractures of the spine; and yet its value can scarcely be overrated in the management of many cases of that accident.

It does not fall within Mr. Fergusson's plan to consider wounds of the thorax and abdomen, and he merely makes the most general possible allusion to them, especially the latter. The following passage respecting wounds of the abdomen is equally applicable to the description of notice taken by Mr. Fergusson of wounds of the thorax:

"I maintain that in this department, as in all others pertaining to surgery, the properly-educated surgeon should here have his knowledge intuitively, as it were. A fixed rule for all such cases cannot be laid down; the management must be left to the discretion of the surgeon, and a reliance upon the general principles of surgery will be more likely to produce good results than a reliance on a dogma which may possibly not be applicable to the case. Perhaps I may err in thus referring to such injuries, and possibly this may arise from my want of experience in cases of the kind. I believe I am not wrong, however, in stating that instances of the sort occur but rarely in civil practice, and when they do come under notice, the circumstances are usually such as need no special directions beyond those precepts which belong to a knowledge of the science of surgery, founded on anatomy and physiology." (p. 708.)

Rational and scientific practice must, unquestionably, be founded on sound general principles; but it is equally true that an important, nay, an indispensable, element of successful practice lies in a knowledge of details—that is to say, in knowing how to apply general principles in particular cases, and when to deviate from general principles under peculiar circumstances. But how is the most gifted student to lay the basis of that familiarity, not only with general principles, but with their application also, which, with accomplished surgeons like Mr. Fergusson, ultimately ripens into something occasionally resembling intuition? One of the most important means of so doing consists in the oral and written instruction of such men as Mr. Fergusson; and as comparatively few can have the advantage of the former, it were to be wished that Mr. Fergusson had indicated to the student the connecting links between principles and practice in these particular cases.

Mr. Miller, in enumerating the signs of intra-thoracic hæmorrhage, says: "The patient lies only on the affected side, and the corresponding cheek has often been observed of a purple colour." (p. 316.) We acknowledge we were not previously aware of the latter sign of intra-thoracic hæmorrhage; but a purple discoloration of the integuments over and below the most depending part of the thorax, resulting from transudation of blood—Valentin's sign, in short, as it is called—has often been observed in those cases.

In the treatment of hæmorrhage into the pleura from a penetrating wound, Mr. Miller says:—"The wound is to be kept open, means are to be

taken to arrest the hæmorrhage at its source, and at the same time to assist the respiration." (p. 317.) All very well if the hæmorrhage comes from an accessible source, such as the intercostal artery, for example. But if the source of the hæmorrhage should be inaccessible, if the lung itself were wounded and bleeding freely, is it not the received and well-founded opinion, that so long as the blood can find a passage through the external wound, the bleeding is likely to continue; and that the best chance of arresting the hæmorrhage is had by closing the external wound as accurately as possible, so that the blood, being confined in the pleural cavity, may compress the bleeding vessels, and act like a plug, as it were, in arresting the hæmorrhage? It is true, as Mr. Fergusson so well remarks in the passage last extracted from his work, that no "fixed rule" can apply to every case alike. If the fixed rule fails in its application, the surgeon must, if he can, devise some new application of general principles, or find resource in his anatomical or physiological knowledge. It was thus that, in a case of penetrating wound of the thorax, involving the lung, when death appeared imminently impending from excessive hæmoptysis and bleeding into the pleura (the external wound being closed), M. Duret, of Brest, considering the difference between a small aperture in the parietes of the thorax, merely sufficient to let the blood effused in the pleural cavity escape, and a free opening, which would probably produce immediate collapse of the lung, and consequent contraction of its vessels and diminution of the quantity of blood circulating through it, enlarged the external wound, in the direction of the intercostal space, to the extent of three inches. The lung collapsed; the hæmorrhage ceased on the instant; and the patient recovered.

The operation for establishing an artificial anus having, of late years, attracted a good deal of attention, a statement of the opinions held by Mr. Miller and by Mr. Fergusson on the subject will probably be acceptable. Mr. Fergusson says on this head (the first part of the extract, it is scarcely necessary to premise, relates to congenital imperforate anus):

"When the infant cannot be relieved by an incision in the perineum, the sigmoid flexure of the colon, or any other portion of the large intestine, which may happen to be prominent, may be cut into through an opening in front, as was originally proposed by Littré. The descending colon, as was recommended by Collison, perhaps even the sigmoid flexure, may be reached behind where not covered by the peritoneum, and thus that membrane may be avoided; but such proceedings have been attended with indifferent success, and considering the condition in which the patient is afterwards left, with an artificial anus in the side, constantly permitting the escape of the contents of the bowels, fatal results are scarcely to be regretted. I have repeatedly, now, on being consulted in such cases, stated the particulars fairly to the male parent, and have left him to decide; the decision has invariably been against any operation under such circumstances.

"In the adult the lower part of the intestine, in some individuals, becomes completely obstructed, by the contraction of a stricture, or in the progress of scirrhus and cancer of the rectum. Amussat, in such a case, has strongly recommended the formation of an artificial anus higher up, and has succeeded in relieving several patients in this way. Mr. Teale, of Leeds, Mr. Alfred Jukes, of Birmingham, and others, have performed such operations. The latter gentleman has published some drawings of the parts, in an instance where he opened the descending colon behind the peritoneum in the lower part of the lumbar region; his patient died on the sixteenth day; and one upon whom Mr. Teale operated in March, 1842, died on the seventh day after.

"This subject has been extensively brought under the notice of the Royal Medico-Chirurgical Society, by Mr. Cæsar Hawkins and others, during the present session, 1851-2, and the success of such an operation, though of a temporary character, is such as might lead one unhesitatingly to resort to such a proceeding in all cases which seem to demand this mode of treatment." (pp. 740, 741.)

Mr. Miller agrees so nearly with Mr. Fergusson that his observations need not be quoted in full, especially as the extract would be a very long one. In congenital imperforate anus, if the passage cannot be restored by an operation performed in the perineum, Mr. Miller, like Mr. Fergusson, deprecates forming an artificial anus elsewhere, thinking "it were better to leave such [children] to perish by the original obstruction of the bowels, than to force on them a more miserable and scarcely less brief period of existence." (p. 394.) In the case of adults affected with malignant disease of the rectum, Mr. Miller would leave the decision to the patient after "having had the danger of the operation, and the almost disgusting result of its success explained to him." In insuperable obstruction from non-malignant disease of the rectum, Mr. Miller thinks "the expediency of the operation may be safely urged upon the patient." Mr. Fergusson, it will have been observed, says, "*Perhaps* even the sigmoid flexure of the colon may be reached behind." Mr. Miller goes further, and asserts, that "the sigmoid flexure of the colon is *plainly* the part of the intestinal canal to be reached, and it may be opened either before or *behind*." (p. 395.) Mr. Miller expresses no decided opinion respecting the relative merits of Littré's and Callisen's (or Amussat's) operations. The former, he says, is more dangerous, and leaves the patient in a condition more offensive both to himself and to others; but, on the other hand, it is the simpler and easier proceeding, and, after some time, the opening gets "something of a sphincteral power," and the escape of fæces may be prevented by a good truss, which the patient can easily manage himself. The only objections urged by Mr. Miller against M. Amussat's operation are—that the opening is liable to contract inconveniently, and that it is out of the reach of the patient; while the proceeding is admitted to be safer, less offensive in its results, and less liable to the risk of prolapse of the bowel.

The exceedingly fair and judicious parallel drawn by Mr. Miller between Littré's and Callisen's operations, seems decidedly in favour of the latter proceeding. But, so far as we can collect, Mr. Miller thinks the balance between them is pretty nearly equipoised, if it does not, indeed, rather incline in favour of Littré's method. We cannot but think that the one element, of Callisen's operation, being admittedly the less dangerous to life, should alone outweigh every other consideration, even if nothing more could be alleged in its favour; but, in addition to that, Callisen's method has the comparatively minor, but positively very important, advantage of greatly diminishing, and, in some instances, almost completely exempting the patient from the loathsome annoyances experienced by patients with artificial anus on the anterior parietes of the abdomen. Mr. Fergusson and Mr. Miller have, perhaps, been somewhat hasty in condemning Callisen's operation in congenital imperforate anus, when relief cannot be given by incisions in the perineum. We are not, indeed, aware whether any of the more recent operations of the kind proved ultimately successful; but it is known that, in at least one instance, the result was favourable.* The indi-

vidual mentioned by Sanson attained the age of manhood, and was capable of engaging in the occupations, probably laborious, incident to his humble position in life. As to endeavouring to reach the sigmoid flexure of the colon from behind without injuring the peritoneum, the suggestion is, we believe, novel, and not very likely to be acted on. Indeed, bating an anomalous condition of the part, the thing would be quite impracticable.

In considering stricture of the urethra, Mr. Fergusson apprehends he is venturing "on ticklish ground, for most writers on stricture seem to be particularly pugnacious." (p. 191.) Mr. Fergusson is not obstinately wedded to any favourite method of treating the disease, but varies his practice according to the circumstances and varying phases of each particular case. Progressive dilatation is, he considers, the preferable plan in the majority of cases; but "the method by cauterization," he thinks, "is perhaps too much neglected in the present day." The lunar caustic, Mr. Fergusson thinks, is probably useful by merely allaying irritability, "but the caustic potash, besides this, undoubtedly destroys tissue, and so permits a more rapid access along the urethra with a large instrument." (p. 786.) Mr. Fergusson also frequently incises strictures in the anterior part of the canal, and occasionally scarifies them all round with "excellent effect;" but "in the deeper part of the urethra, this plan" (says Mr. Fergusson,) "is not to be recommended, and should be ventured on solely by one fully acquainted with the anatomy of the parts." (p. 791.) As regards this practice, Mr. Miller agrees with Mr. Fergusson that "for very tight and unyielding contractions anterior to the suspensory ligament, the method is not unsuitable, though, even there, it is not unattended with some risk of infiltration of urine through the cut parts; but Mr. Miller, very properly, in our opinion, altogether condemns the use of the lanceted catheter deep in the urethra, "under any circumstances whatever." (p. 528.) Mr. Miller tolerates the use of the caustic bougie "as a corrector of irritability;" but, with respect to its use as a caustic proper, he makes the following observations, than which nothing could be more sound and judicious.

"To prove successful as an escharotic, in clearing away obstruction, the mucous membrane must first be sacrificed; and though, for a time, ample space may thus be obtained, yet in the end recontraction is obviously inevitable; partly by reason of the plastic deposit which surrounds ulceration, and partly by reason of the contraction which invariably attends on cicatrization of a sore." (p 529.)

Mr. Miller considers the "perineal section" in the treatment of stricture of the urethra very briefly, but very fairly and judiciously. He believes, as almost all surgeons do, that "some few strictures are really impermeable;" and in such strictures Mr. Syme's particular operation cannot, of course, be performed. On the other hand, Mr. Miller is persuaded that the vast majority of 'penetrable' strictures can be well and safely cured without the use of cutting instruments. But some few cases of permeable stricture do prove intractable under the ordinary method of treatment, and in such cases Mr. Miller thinks that Mr. Syme's operation is "very suitable." (p. 530.)

Mr. Fergusson's estimate of the "perineal section" appears in the following extract, which is the more interesting, as in it are stated some facts, derived from Mr. Fergusson's own experience, respecting the immediate and ultimate results of an operation, which has unhappily occasioned so much and such embittered controversy.

"Every surgeon of experience must have met with examples of stricture, with little or no indication of mischief otherwise in the perineum, where the bougie has been productive of mischief, or where probably the disease, having been temporarily improved, has speedily returned, and where again and again there have been the same treatment and results. It is chiefly in such cases that the division of the stricture has been recommended, and in advocating the operation Mr. Syme sets down as fundamental rules for its due performance certain doctrines totally at variance with those generally held by surgeons. An instrument must be passed through the stricture, otherwise the perineal section cannot be performed. Surgeons have heretofore considered this the commencement of certain success with the after use of the bougie. Mr. Syme makes it essential that an instrument, however small, should be passed through the stricture, to guide the knife and to insure that the canal is laid open, and not any false passage that may be near it. To those who ask what is to be done when an instrument cannot be introduced? Mr. Syme replies, that there is no such thing as an impermeable stricture, and that it is only the surgeon's want of skill which prevents the proper introduction. Every-day experience shows that one surgeon will introduce an instrument where another has not succeeded, but there are few who maintain that they never fail in this proceeding, and it is certain that the most expert operators have failed to pass instruments where there has been positive proof at the time, by the dribbling of the water, that the canal has been in some degree permeable. . . . . .

"Mr. Syme's reports of this practice, as published in the 'Edinburgh Monthly Journal' and in the 'Lancet,' are most favourable, and I have, myself, repeatedly seen the best results follow, but, like all other cutting operations, it is not devoid of danger. Several deaths from it have been recorded, and I have myself had one fatal example, the patient sinking soon after the operation without any other seeming cause than the wound. I have seen alarming hæmorrhage, and one young man nearly lost from this cause, which continued from a wound in the bulb for twelve days, and brought the patient to the lowest ebb of life. No such mishaps have occurred in Mr. Syme's hands, however, although various other surgeons have not been so fortunate. . . . . From my own observation I can positively state that this mode of division of the stricture with the knife is not always followed by the entire relief from after annoyance that some have been led to expect, and whilst I think highly of the practice in certain instances, I am of opinion that, like all others in surgery wherein the knife is required, it is not free from hazards, over which the surgeon has no control." (pp. 787—789.)

Neither Mr. Miller nor Mr. Fergusson mention Mr. Colles' operation for the permanent cure of stricture at the orifice of the urethra consequent upon ulceration surrounding the outlet of the canal; a form of stricture almost uniformly unmanageable by any of the methods previously employed. The accidents that are liable to occur during the treatment of stricture are very slightly noticed both by Mr. Miller and Mr. Fergusson, but the rigors, heat, and sweating, that so frequently supervene after the passage of an instrument, are thus alluded to by Mr. Miller:

"Other patients are liable to suffer from agueish attacks, after the use of bougies; such are generally elderly persons, who have lived freely and been abroad. They benefit greatly by the use of quinine." (p. 526.)

It is needless to dwell upon the radical distinction between urinary fever and ague; or to observe that opium, whether as a preventive or curative means, is greatly preferable to quinine in these attacks, which cannot be termed agueish; although they have, indeed, been occasionally mistaken for remittent fever, because of their presenting a cold, hot, and sweating stage with tolerable regularity.

We at first read with something more than surprise the following passage in Mr. Fergusson's observations on the treatment of aneurism:

"In the practice of surgery, the application of a ligature to the vessel with which the disease is connected, has a powerful and specific effect; and although the operation for its accomplishment is not without danger, both at the time and subsequently, it possesses such advantages over all other modes of treatment, that the surgeon who, when other circumstances are favourable, hesitates to adopt or recommend it, may with justice have either his skill or his sincerity called in question." (p. 145.)

On reading further, however, it appeared that the preceding passage had been allowed to stand as it appeared in the original edition of the work, for the purpose, it may be presumed, of giving additional weight, as undoubtedly it does, to Mr. Fergusson's present opinion respecting the comparative merits of ligature and compression of the artery, in cases where the latter proceeding is practicable. What Mr. Fergusson's opinion *was*, we have just seen; what his opinion now *is*, may be learned from the following extracts. The quotations we are about to make are of considerable length, but Mr. Fergusson occupies a foremost place as a dexterous, and what is of greatly more value, a scientific and sagacious surgeon; and his matured judgment respecting one of the most momentous questions in modern surgery will be read with interest, and carry with it the weight it is so well entitled to.

"Since these remarks were published in the last edition of this work, the treatment of aneurism by compression has attracted further attention, and the results, in the hands of the Dublin surgeons especially, have placed the success of the practice beyond doubt. Much credit is due to Dr. Bellingham, of Dublin, for the attention he has paid to this interesting subject, and Mr. Tuffnell, of the same city, is not less worthy of praise. The latter gentleman has published a most admirable treatise upon it; and Dr. Bellingham's most recent views have been published, in a short paper on the Treatment of Popliteal Aneurism by Compression, in the thirty-fourth volume of the 'Transactions of the Royal Medical and Chirurgical Society, 1851.' From this paper it appears that 36 cases of external aneurism had been treated in this manner in Dublin, during the preceding seven years. To use Dr. Bellingham's own words, 'In 29 of these a cure was effected by compression; of the remaining seven cases the artery was tied in two, the patients recovering. In one, pressure was discontinued, the aneurism subsequently diminished in size, and the patient had the perfect use of the limb for three years, when symptoms of aneurism of the aorta supervened, and compelled him to give up his employment. In two, the limb was amputated, the patients recovering; and in the remaining two, death occurred, in one from pulmonary disease, in the other from a severe attack of erysipelas; but, in both, the local disease was very nearly cured, the aneurismal sacs being almost completely filled by fibrine deposited in concentric layers.' In not one of these cases can it be said that evil resulted from pressure. If erysipelas was the result of the interference of the surgeon, it was more likely the sequence of the galvano-puncture, which was resorted to in this instance in conjunction with compression, than from the pressure. These 36 cases were treated by 21 different surgeons, and both numbers give fair practical inference of what may be expected from a continuance of the practice among surgeons generally. The method has been tried frequently by other than the Dublin surgeons, but as yet we have no data from their cases to form any positive conclusions regarding it. Many instances have been mentioned to me wherein it has failed, but I have heard of many more in which it has been successful; and if Dr. Bellingham's Table—which, as we learn from the 'Dublin Medical Press,' for December 3, 1851, has since been increased to 62 cases—be contrasted with some

of those showing the results of ligature of the femoral artery in the hands of various surgeons, the balance seems greatly in favour of compression. It has been ascertained by Dr. Norris, that of 188 cases, in which the operation was performed, 46 died, the majority of this number being from causes directly attributable to the use of the knife. In six of the cases of recovery in the above list, amputation of the limb was required. Of 119 cases of popliteal aneurism, collected in a tabular form by Dr. Crisp, 16 died, and in six of those wherein recovery took place, amputation was resorted to. So far as our comparatively limited experience in the method by pressure, as followed by the Dublin surgeons, will enable us to form an estimate of its value, it seems in many respects, if not in all, preferable to that by deligation of the main artery; and there seems these great advantages in it, that if it does not act satisfactorily, the Hunterian operation may still be resorted to with as much probability of success as ever, while by its application none of those formidable dangers are incurred, which are the well-known consequences of the application of the ligature. The difficulties and immediate dangers of a cutting operation are avoided. It may seem strange to make use of such language in the present day, as applicable to ligature of the superficial femoral artery—for that, I assume, to be the vessel meant by the unfortunately vague term of 'femoral;' nevertheless, when it is known that great difficulties have been experienced in such a proceeding, even by hospital surgeons and teachers of surgery, and that the accompanying great vein has been wounded by different operators, the facts cannot be overlooked. Mr. Syme states in the 'Edinburgh Monthly Journal of Medical Science,' for November, 1851, that he has tied the superficial femoral artery twenty times without a fatal issue, and with perfect success, and both he and his patients may be congratulated on such satisfactory results; but the tables above referred to show no such average success: and when it is borne in mind that the attempt at cure by pressure does not preclude the resource of the ligature, it seems to me that, with the ample evidence before us of the great success which has attended the modern practice by this means, the surgeon should undoubtedly give it trial ere he resorts to the knife. Unquestionably the evil result of the Hunterian operation, in regard to ligature of the superficial femoral artery, has in many instances resulted from the defective or injurious style of operation, but the same may be said of certain examples where pressure has failed. Granting both of these statements to be correct, it cannot be overlooked that ligature of the superficial femoral artery, done to the perfection of human skill, has nevertheless been followed by the worst possible results. I have seen Mr. Syme perform the operation repeatedly with admirable skill and precision in all points, and the results have been all that could be desired; but I have seen many others, and among them I may name the late Mr. Liston, perform the same operation with an equal amount of tact and judgment, yet the results have been very different. With pressure, surgery has still further resources, but with the ligature the fate of the case is, for a time, placed almost, if not quite, beyond human power; and doubtless the surgeons of Dublin who have resorted to this practice (many of whom stand among the highest of those who have graced the annals of the profession) have duly considered all these points." (pp. 150—152.)

"For either the popliteal or femoral aneurism the pressure may be made on the common femoral artery, or on the superficial. Sometimes one point may be chosen, sometimes another, and not unfrequently in one case both may be tried alternately.

"The principal feature in all the instruments which have been used for such practice is, that, while effectual pressure may be kept up on the main artery, the collateral branches throughout the greater part of the limb have free action, and the part below is sufficiently supplied with blood. Instruments similar to those which were used in the last century have been revived, and some modern improvements have also been called into play. I have seen the tourniquet of Signoroni (see p. 34) used with admirable effect; and two of the successful cases in the

tables given by Dr. Bellingham were treated under my observation by my friend and former pupil, Mr. Robert Storks, with this instrument alone. But it is difficult to keep the pad steady on one point, and an instrument which fairly encircles the limb, without, however, compressing all the surface, is to be preferred. . . . . An ingenious addition to the common screw force has been applied here by means of bands of caoutchouc, whereby a certain resiliency is acquired, which, while it keeps up effectual pressure, may possibly obviate injurious effects from the screw. . . . . Whatever instrument is used, it is of importance to bear in mind that the absolute stoppage of the circulation is not essential, as it has been proved by experience, that a diminished force of circulation is sufficient to effect the desired end. If, however, it is found that pressure strong enough to arrest the flow of blood in the main vessel can be applied, then doubtless the favourable effects may be expected more speedily." (pp. 433, 434.)

Mr. Miller's observations on the same subject well deserve to be quoted along with those of Mr. Fergusson, and, like them, supply an excellent example of sound and candid surgical reasoning.

"*The Popliteal* is probably the most common of all external aneurisms; and, hitherto, the Hunterian application of ligature, to the superficial femoral, has been the only approved mode of treatment. Latterly, however, as elsewhere explained (Principles, p. 580), the application of pressure, instead of the ligature, has been employed. And experience is, almost daily, giving direct and undoubted testimony to the efficacy of the practice. There are some patients, doubtless, who may prove intolerant of pressure; and there may be others who prefer the apparent certainty of the knife and ligature, to the apparent uncertainty and delay of the compressor. But the greater number of cases are assuredly capable of cure by pressure properly applied; without risk, with but little pain or inconvenience, and without any wearisome amount of privation or confinement. The skin, which is to bear the pressure of the instrument, is protected by a layer of thick soap-plaster; and that, again, may be covered by leather. More than one compressor is used; or, at least, pressure is made at different parts, at different times; so that the burden of it may not all be thrown on one point, but, by being subdivided, may be rendered more tolerable. Using several instruments, along the course of the vessel in the thigh—they may be slackened and tightened alternately; or the same instrument may be shifted in its site, with a like effect. It is never to be forgotten, that all severity of pressure is unnecessary; and that it is not essential to arrest the arterial flow, at the compressed point. And it is also important to remember, that should this mode of treatment fail, it by no means interferes with the subsequent performance of the ordinary operation; but, on the contrary, the constitutional treatment suitable for pressure renders the success of subsequent deligation all the more probable. Those surgeons who obstinately adhere to the old operation may adduce, as their apology, a series of successful cases so treated. But this is very plainly a contracted view of the subject; and as well might such practitioners prefer successful amputation of the hand to amputation of a finger, for a simple affection of the latter only. A surgeon of the olden time, who had succeeded in curing several successive cases of popliteal aneurism by amputation of the thigh, might very naturally entertain a distrust and dislike of the proposal to treat the same disease by ligature of the femoral; but the naturalness of such an aversion to the minor and modern practice, would not render it one whit the more reasonable or praiseworthy. And an impartial observer will not consider any one justified, in subjecting his patient to serious risk of life, by hæmorrhage, suppuration, and gangrene; while he has it in his power to effect a cure by a minor means, comparatively devoid of risk, and the failure of which will not militate against subsequent recourse to the major procedure—if necessary. Why should a mode of treatment, which causes little or no risk, always be passed by; or why should an operation always be had recourse to, which may, and not unfrequently does, result in direct loss of life? And the question comes in much force, if it be admitted—

and statistics will scarcely warrant even feeble contradiction of this any longer—that the two methods are at least equally successful for the cure of aneurism.

"Recorded facts seem to prove the following conclusions:—1, That, in popliteal aneurism, skilful compression of the femoral is capable of curing the disease, and that with comparative, and almost absolute safety to life and limb; 2, That the time expended in cure is, on an average, not greater than in the treatment by ligature; 3, That failure by compression does not compromise subsequent recourse to deligation; 4, And that, consequently, compression, when skilfully employed, being equally certain, far more safe, and not more tedious than the ligature, should in the great majority of cases be preferred. The only disadvantage of compression is the care and trouble necessary on the part of the attendant, with irksomeness and sometimes suffering on the part of the patient. The obvious and only advantage of deligation, on the other hand, is the facility and dispatch of its execution, with probable exemption from suffering afterwards by the patient, in the successful cases. The formidable disadvantage is, its proved risk to life and limb. (pp. 594—596.)

Our extracts must terminate here, though pages might readily be filled with quotations as creditable to Mr. Miller and Mr. Fergusson as they would, doubtless, be interesting to our readers. It were superfluous to express any opinion respecting the general merits of the works before us; for we could only express our concurrence in the favourable verdict long since pronounced by the profession, and we do not affect to suppose that we could add any weight to the decision of the authority most competent in the matter. At a time when new books daily fall still-born from the press, Mr. Miller's and Mr. Fergusson's works have, respectively, reached their second and third editions, and no eulogium need be added to such tangible evidence of substantial and acknowledged merit. It is only necessary to say, that these publications are not mere reprints; they are new editions in the proper sense of the term, a large amount of new and valuable matter having been added to both works, to bring them fully up to the existing state of science. Both works, it need scarcely be added, are written with the same generous and candid tolerance of the opinions of others which distinguished the former editions, and which stands in such strong and favourable contrast with the style of some surgical writers of the present day.

R. C. Williams.

REVIEW VI.

Lehrbuch der physiologischen Chemie. Von Prof. Dr. C. G. LEHMANN. Dritter Band.

Physiological Chemistry. By PROFESSOR LEHMANN. Third volume. —*Leipzig*, 1852. 8vo, pp. 518.

LEHMANN'S 'Physiological Chemistry' is now completed, and the third and concluding volume, which now lies before us, is in no respect inferior to its two predecessors, which were noticed at some length in the recent numbers of this journal. The first and second volumes were respectively devoted to "the organic substrata of the animal organism," and to "the animal juices;" the third volume embraces two distinct subjects—namely, "Histochemistry," and "Zoochemical Processes."

In the introduction to the "Histochemistry," we have a brief general

sketch of the leading steps by which this department of science has been advanced to its yet very imperfect state. Müller was the first to adopt the plan of examining the action of chemical re-agents in microscopic preparations. This method in the hands of Mulder and Donders, and of other experimentalists, has already led to valuable results. Thus, it was only by this method of microscopico-chemical analysis, that we first attained any clear idea of the structure of the different varieties of horny tissue, or that all doubt regarding the axis-cylinder of the nervous fibres was removed. The numerous sources of fallacy, and the precautions by which they may be (as much as possible) avoided, are well pointed out in these introductory remarks, which are concluded by an attempt to establish the law that *the chemical nature of the tissue always corresponds to its function.* Our author endeavours to deduce this law from the following considerations. It has been a long recognised fact, that every tissue which is serviceable to the body almost solely from its physical properties, as its hardness, toughness, pliancy, &c., contains, as its most essential substratum, a substance which on boiling yields gelatine; we know, moreover, that those textural elements, which are especially distinguished by a high degree of elasticity, as the nucleated fibres of areolar tissue, and the true elastic tissue, exhibit perfectly similar chemical re-actions; and (as Lehmann proceeds to show in a subsequent part of the volume) all the more vitally active tissues—those namely, which, in addition to a certain amount of elasticity, possess the property of contracting in consequence of certain influences transmitted to them by the nerves—contain, as their most essential constituent, muscle-fibrin or syntonin; moreover, the mode of arrangement, and the chemical characters of the substrata constituting the nervous system, confirm the truth of the law, that the materials of which the tissues are built up present a chemical conformity with their vital capacities.

The *osseous tissue* is the first of the individual textures treated of by our author. In his microscopical description he almost entirely follows Kölliker; in the chemical department he freely avails himself of the labours of von Bibra, Stark, Ragsky, Marchand, Nasse, &c. In addition to a full account of the chemistry of the bones of man, and the different classes of animals, we have an excellent abstract of all that is at present known regarding the chemical changes occurring in primary sclerosis osteophyte, exostosis, osteoporosis (dilatation of the medullary cells and of the Haversian canals), osteomalacea, including the craniotabes of Elsässer which has been so admirably investigated by Schlossberger, in caries and in necrosis. There is nothing, however, in this section, or in the followin[illegible], *on the teeth*, specially demanding notice.

Cartilage, areolar and *elastic tissues, horny tissue*, and *hair*, are duly considered in their appropriate sections.

Contractile fibre-cells—a term probably new to many of our readers—next claim our attention. For most of our knowledge of these cells, which in a condition of aggregation have been known as organic or smooth (unstriped) muscular fibre, we are indebted to Kölliker. They usually occur as elongated, thin fibres, with pointed extremities, but sometimes as four-sided or club-shaped plates, with occasionally jagged edges. They usually present a distinct nucleus, which becomes more apparent on the addition of acetic acid. The substance of the cell sometimes exhibits granules

linearly arranged along the axis of the fibres, but in other respects is homogeneous. These fibre-cells, when arranged laterally, form the bundles of smooth or unstriped muscle which are recognisable even with the naked eye in the intestinal canal. Kölliker makes two divisions of *pure* and *mixed* unstriped muscle; in the former the cells are closely grouped together, so as to form bundles, or even membranous patches; in the latter they are only scattered over other tissues; the former occurs in the muscular coat of the lower half of the œsophagus, and in the muscular coat of the stomach and intestinal canal, in the nipple, the prostate, and the vagina; the latter is most obvious in the trabecular tissue of the spleen, but may also be seen in the tunica dartos, in the middle arterial coat, in veins and lymphatics, in the corpora cavernosa, the prostate, the Fallopian tubes, the uterus, and the urethra; in the trachea, the bronchi, ureters, and vasa deferentia, the tissue presents an intermediate character approximating to the pure type. Both these forms are more or less imbedded in, and intermixed with, areolar tissue. The unstriped muscles are never enclosed in a true sarcolemma.

The use of these textural forms does not depend, like that of the elastic fibres, with which they are so often associated, on their physical character; but it is to them that the tissues in which they occur owe their contractility, under the influence of the nervous system. Ed. Weber, in his admirable article on "muscular motion," in the third volume of 'Wagner's Handwörterbuch der Physiologie,' has clearly established the differences of action between the contractile organs and the voluntary muscles. He has found that almost all organs provided with these fibre-cells undergo a very gradual, and, at first, a very limited contraction, when a mechanical or chemical irritant is applied to them. He chiefly employed a galvanic current obtained from a rotation machine, and he found that the movement very gradually passed from the nearer bundles of fibres to the more remote ones, and did not disappear for a considerable time.

The micro-chemical reactions of acetic, hydrochloric, sulphuric, nitric, chromic, and phosphoric acids, and of solutions of alkalies and alkaline salts, in various stages of dilution, on these fibre-cells, have been carefully studied by Donders,* Schultze,† Paulsen,‡ and Lehmann. From these experiments it appears that the substance of which these fibre-cells are composed closely resembles, although it is not identical with, the fibrin of the blood.

Lehmann observes that much importance should probably be attached to the circumstance, that those organs which present most vital activity are moistened by a fluid, which is very different from an ordinary transudation, or from the plasma of the blood. Liebig's discoveries have shown that the striped muscular fibres are surrounded by a fluid very different from the blood-plasma, and the same is the case with the fibre-cells of the unstriped muscles. Thus Schultze, in examining the middle arterial coat, found that it was permeated by a fluid which was very rich in casein. In 100 parts of the dried circular fibrous coat of the thoracic aorta, he found 23·1 of soluble constituents, of which 7·24 were casein; while in the middle coat of the carotid, which contains fewer elastic fibres, but far more

* Holländische Beiträge, and Mulder's Vers. einer phys. Chem.

† Ann. d. Ch. u. Pharm vol. lxxi. p. 277.

‡ Observationes Microchemicæ; diss. inaug. Dorpati, Liv. 1848.

contractile fibre-cells than the aorta, he found 39 p. c. of soluble constituents, of which 21·were casein. This interstitial fluid had a faint alkaline reaction, and besides casein and salts, contained small quantities both of a substance coagulable by heat, and of a non-coagulable substance. Lehmann himself found that the fluid which moistens the muscular coat of the stomach of the pig has a distinctly acid reaction, although less intense than the juice of striated muscle; while the analogous fluid of the middle arterial coat (of different parts of the aorta and carotid of the ox), only faintly reddened litmus, and that from the tunica dartos exerted no action on vegetable colours. He endeavours to account for the circumstance of Schultze finding the juice of the middle arterial coat alkaline, by the supposition that it was mixed with some of the alkaline fluid yielded by the areolar tissue, or that decomposition had commenced in it. There is more casein and less albumen in the middle arterial coat and in the tunica dartos than in the muscular texture of the pig's stomach; the latter is also as rich in albumen as the juice of the voluntary muscles.

Creatine and inosite are contained in this juice in far less quantities than in the juice of the striated muscles; indeed, it is only by crystallometric admeasurements, made with the microscope, that we can convince ourselves of their presence. Lactic, acetic, and butyric acids are also present in minute quantities. The ratio of potash to soda was = 38:62 in the juice of the unstriped muscle of the stomach, and = 42:58 in that of the middle arterial coat; the ratio of the soluble to the insoluble phosphates = 82:18 in the muscular juice from the stomach, and = 79:21 in that from the middle arterial coat.

From these relations, and from others, fully described by Lehmann, but which our limited space forbids us to notice, it follows that there is, at all events, a very close analogy between the juice of these fibre-cells and that of striated muscular fibre. Even if we entertain any doubts (as some of our leading histologists still do) regarding the existence of the fibre-cells in the middle arterial coat, there is unquestionable chemical evidence that the striated and the unstriped muscles, and the contractile tissues, not only contain a solid material which is chemically identical in all three, but also that they are bathed by a fluid which differs essentially from all other animal juices in its acid reaction, its abundance of potash-salts and phosphates, and in its containing creatine, inosite, &c.

We regret that we have no space to notice the microchemical investigation by which it is proved that the three morphotic elements which occur in *striated muscular fibre*—namely, the substance of the fibrillæ, the substance of the nucleus, and the sarcolemma, are chemically different from one another; we must, however, extract Lehmann's remarks on muscle-fibrin, or syntonine, the true substance of the fibrillæ:

"From the preceding micro-chemical investigations, we must agree with Liebig in regarding the material extractable from the muscles by dilute hydrochloric acid as the true elementary substance of true muscular fibre. We have already remarked that this material, although it possesses the most essential properties of the protein bodies, and in many respects comports itself like blood-fibrin, is not identical with it. The muscle-fibrin or syntonin extracted from striated muscles by diluted hydrochloric acid, and precipitated from the solution by neutralization of the acid, forms, while moist upon the filter, a coherent, somewhat elastic, snow-white mass, which may be detached from the filter in plates or membranes;

on applying tension to the more delicate plates, they present under the microscope a fibrous appearance not unlike blood-fibrin. This substance, while still moist, is very readily soluble in lime-water, as well as in dilute alkalies; it coagulates from its lime-water solution on boiling, like albumen; and is precipitated both from this and the alkaline solutions by concentrated solutions of neutral alkaline salts; when placed in a moderately concentrated solution of carbonate of potash, the mass swells, becomes gelatinous and opaque, but does not dissolve; it is only after very considerable dilution that any solution commences. In this respect it perfectly coincides with the experiments I have made with carbonate of potash on flesh in large quantities, and likewise in the micro-chemical examination of the fibrillæ of primitive fibre. On adding chloride of calcium, or sulphate of magnesia, to the alkaline solutions of this substance, no precipitate is thrown down, unless the mixture be boiled; if, however, the alkaline solution be previously boiled (which at most renders it opaque), solutions of the above-named salts at once induce a flocculent precipitate. Nitric acid throws down a white flocculent precipitate from the alkaline solutions of syntonin. Chromic acid, or acid chromate of potash, and hydrochloric acid, precipitate this substance in flakes, both from its alkaline and acid solutions; pure hydrochloric acid, if added in excess, merely induces a turbidity in the alkaline solution.. Uncoagulated syntonin does not dissolve in a solution of nitre (6 parts of KO,NO_5 in 100 of water) even after five days' digestion at 86° Fahr. It has been already mentioned that the primitive muscular bundles, even when digested for a longer period with a solution of nitre at from 86° to 104°, present no change under the microscope, which would lead to the supposition that there was even a partial solution of the finest muscular fibrillæ. Since, however, the above experiments were, for the most part, made with beef and veal, and, as is well known, the fibrin of ox-blood is almost perfectly insoluble in a solution of nitre, while that of other animals dissolves readily when digested with that fluid, other experiments were made with pork, from which the fat was thoroughly removed, the meat being finely chopped, and treated with distilled water till the expressed fluid no longer contained traces of albumen. This flesh-mass thus perfectly freed from soluble protein-compounds, was digested from two to five days, in a solution of nitre of the above strength; but this fluid did not dissolve even a trace of a protein-compound, coagulable by heat or acetic acid, or precipitable by any other reagent. The syntonin naturally contained in the fibrillæ of muscle is therefore just as insoluble in a solution of nitre, as the syntonin artificially obtained from the muscles by hydrochloric acid." (vol. iii. pp. 86, 87.)

Strecker's analyses show that this substance differs slightly in its ultimate composition from blood-fibrin.

The colour of the muscles is usually supposed to depend upon the quantity of blood which they contain. Lehmann, however, inclines to Kölliker's opinion, who believes that the muscles possess a pigment of their own, which is very similar to, but not identical with, hæmatin. Kölliker's argument, that this pigment is not contained in the vessels and blood-corpuscles, but adheres to the fibrillæ, is founded, not so much on chemical as on the following physiological grounds. During contraction, the muscles retain their colour; comparatively colourless muscles are often as rich in bloodvessels as strongly coloured ones; and, lastly, even under the microscope, some bundles may be observed to have a decidedly yellow colour.

We shall conclude this subject with the following remarks on the light which chemical inquiries have thrown on the physiology of muscle:

"The protein-substance extractable from the fibrillæ of muscle by extremely diluted hydrochloric acid, is the most essential element of animal motion; it is

one and the same in the striated muscular fibres, in the smooth muscles, and in those tissues which were formerly termed contractile; it is, however, peculiar to those organs only whose power of movement is dependent on the nervous system. What changes in its physical properties this substance undergoes in the contraction of the tissues, we are unable to explain chemically.

Both in the voluntary and the involuntary muscles there is, moreover, a fluid which bathes and moistens the tissues in which the contractile fibres lie, and is distinguished by its acidity, and altogether different from the plasma of the blood. Liebig has calculated that the voluntary muscles alone contain more than a sufficient quantity of acid to neutralize the alkalinity of the blood. We have seen that wherever contractile fibres or fibre-cells occur, the permeating fluid or juice always contains a preponderating quantity of potash-salts and phosphates, while, on the other hand, the blood-plasma is poor in these salts and rich in alkaline chlorides and soda-salts. The antagonism between the intercellular fluid of the blood-cells and the interstitial fluid of the contractile fibres cannot be a mere accident. Liebig had already shown that this antagonism must give rise to an electric current, or must be occasioned by such a current; and since that time we know that Du Bois-Reymond* has thrown much light on the electrical phenomena of muscular contraction, and obtained many brilliant results, clearing up the preceding labours of Matteucci. It is sufficiently obvious that the development of electricity which accompanies muscular contraction is intimately connected, on the one hand, with the acid of the muscular juice, and on the other, with the alkali of the blood; and that the chemical constitution of the muscular juice is of special importance in relation to the function of the organ, from the well-known and very striking fact, that all muscles, voluntary as well as involuntary, (striated and smooth), very rapidly lose their contractility in water. This experiment, which any one may confirm for himself who has seen contractions induced by a rotation apparatus in the striated muscles, in the stomach, intestinal canal, or bladder of an animal just killed, appears to be opposed to the results obtained by the younger Liebig,† who found that muscles from which the blood had been as completely as possible removed by the injection of water into the vessels, retained their capacity for contraction as long as muscles which contained blood. There is, however, no real contradiction between these two observations; for if the muscles do not lose their contractility on the removal of the blood, but do so after the addition of water, this only proves that they are deprived of their capacity for contracting by the dilution of the muscular juice. To establish a connexion between the development of electricity during contraction with chemical forces, we must seek the causes of the polarity less in the antagonism of the alkali and acid, than in that of the solid substance of the muscular fibrillæ (syntonin) and the muscular juice." (vol. iii. pp. 98, 99.)

As the views of the younger Liebig, referred to in the above extract, may not be familiar to the majority of our readers, we will briefly give his principal results. His most important conclusion is, that a muscle cannot contract unless oxygen be present; he found that the muscles of the frog retained their capacity for contracting in an atmosphere of oxygen, longer than in a non-oxygenous atmosphere; that is to say, than in carbonic acid, nitrogen, or hydrogen. He further ascertained, that as long as the capacity for contraction existed, oxygen was absorbed, and a corresponding quantity of carbonic acid exhaled—facts which establish the view that at all events a great part of the carbonic acid produced in the animal body is formed externally to the capillaries in the parenchyma of organs, and that it is principally dependent on muscular action.

* Unters. üb. thier. Electricität. Berl. 1848-9.

† G. v. Liebig, in Ber. d. Akad. d. Wissensch. zu Berlin, 1850, pp. 339—347.

The succeeding twenty-five pages are devoted to *the Chemistry of the Nerves and Brain;* and although they contain all that is known on the subject (and perhaps something more), they leave much still unexplained.

The first portion of the volume concludes with *Exudations and Pathological Formations.* After alluding to the almost innumerable difficulties which surround this department of zoo-chemistry, he proceeds to point out the dependence of "the phenomenology of the process of exudation" on mechanical conditions, and especially notices "the memoirs of Jolly* and Ludwig† on endosmosis and endosmotic equivalents; C. Schmidt's‡ beautiful investigations regarding the relation of the coefficients of density of saline solutions to the diffusion-equivalents, and the remarkable discoveries of Graham§ on the diffusion of liquids."

Our author adopts Rokitansky's mode of arrangement, and considers exudations under the following heads:

1. As fibrinous, these being subdivided into plastic and croupous.
2. As albuminous; and,
3. As purulent, with which must be classed diffluent and hæmorrhagic exudations.

In connexion with tuberculous exudations, which are a mere subdivision of the fibrinous species, Lehmann observes that cystine has recently been discovered as occurring in old tubercles; he has not, however, had an opportunity of personally confirming the observation. We may here also direct the attention of our readers to the circumstance that Dr. Struthers has recently detected large quantities of crystals of urate of ammonia and triple phosphate in the contents of a pulmonary abscess, consequent on the lodgment (for four years and a half) of a foreign body in the air-passages. "The crystals of urate of ammonia," says Struthers, "were formed, no doubt, on the spot by the destruction and decomposition of the surrounding tissue."‖

We regret that we can afford little more than a passing remark to the excellent section on *Pus.* In accordance with the views of the best recent pathologists, Lehmann believes that no difference whatever exists between pus-corpuscles (or, as Henle terms them, cytoid-corpuscles), lymph-corpuscles, colourless blood-cells, and mucus-corpuscles. He mentions a case of catarrhal icterus, in which one of his pupils discovered glyco-cholate and tauro-cholate of soda in the contents of a large abscess in the leg, and a case of diabetes, in which sugar was found in a purulent discharge.

"The differences," he observes, "between pus and mucus, on which physicians formerly laid so much stress, are no longer of any importance, since a more recent physiology has taught us that there is no line of demarcation between these fluids, and that they gradually merge one into the other, and that the mucus in inflammatory affections of mucous membranes gradually presents large quantities of cytoid-corpuscles, and that at the same time its albumen increases, so that both in a physical and chemical point of view it thoroughly resembles pus, if it be not actually the same thing: even the quantity of fat in the purulent fluid secreted by inflamed mucous membranes—a point which Güterbock regarded as of the greatest diagnostic value—is often precisely the same as in genuine pus: in most cases the pus secreted from mucous membranes retains its mucous property of gelatinising on the addition of water or of acetic acid." (vol. iii. p. 165.)

* Zeitsche. f. rat. Med., vol. vii. pp. 83—148. † Ibid. vol. viii. pp. 1—52.
‡ Charakteristik der Cholera, pp. 22—28. § Transactions of the Royal Society for 1850.
‖ Monthly Journal of Medical Science, Nov. 1852, p. 455.

We have now arrived at the second division of the volume—at the portion treating of *Zoochemical Processes*, which includes the consideration of *molecular forces, the formation of organic matter in the vegetable kingdom, a general review of the metamorphoses occurring in the animal body, digestion, respiration,* and *nutrition.*

Passing without comment over the first two sections, we would direct attention to our author's views regarding the differences which present themselves in the metamorphoses of the two great kingdoms of organic matter. Liebig, in one of his earlier writings, contrasted the functions of plants and animals, in the following terms :

"The PLANT *produces* neutral nitrogenous bodies, fats, sugar, starch, and gum; *decomposes* carbonic acid, water, and salts of ammonia; *developes* oxygen; *absorbs* heat and electricity; is a *reducing apparatus,* and is devoid of the power of motion.

"The ANIMAL *consumes* neutral nitrogenous bodies, fats, starch, sugar, and gum; *produces* carbonic acid, water, and salts of ammonia; *absorbs* oxygen; *developes* heat and electricity; is an *oxidizing apparatus,* and possesses the power of motion."

We shall endeavour to show how far these statements require modification. There can be no doubt that the organic matter which is produced or formed in the vegetable world, is for the most part decomposed or reduced to compounds of a lower order in animals; but the assertion that animals merely consume the protein-compounds, fats, and carbo-hydrates, and cannot in any degree produce them, is not only incapable of proof, but to a certain degree is unquestionably false. It is established beyond all doubt, that flesh can be formed in the animal body, whether the elements from which it be produced are protein-bodies, or carbo-hydrates. (This subject is fully discussed in the first volume; see pp. 254—258 of the English translation.) Moreover, it is still an open question whether, under certain conditions, protein-substances may not be formed in the animal organism (see vol. i. p. 346 of translation of the first volume).

"Although," says Lehmann, in reference to this subject, "if we turn to the lower animals, we must deny that they possess the power of forming starch or cellulose, yet sugar and dextrine are being perpetually formed in the organisms of the herbivora during digestion from other carbo-hydrates, by the action of saliva and pancreatic juice; and even in the bodies of carnivora we have a factory for sugar in the liver, where in all probability this substance is formed solely from the metamorphosis of nitrogenous matters, as careful investigations have recently convinced me. And how many substances there are formed in the animal body, which never occur in the vegetable kingdom! It may, indeed, be urged, that these substances are only the products of a process of oxidation; but what an essential difference there is between xanthine or uric acid, and theine and theobromine, which are homologous to them! And no one unacquainted with the origin of cystine or of taurine, could say whether these substances were products of the animal or the vegetable kingdom We have nothing in the vegetable kingdom analogous to those complex combinations, the salts of the biliary acids; in short, we cannot deny that the animal body possesses the power of forming new compounds; but both in the animal organism and in the laboratory of the chemist, already formed organic matter is requisite for the formation of new substances not occurring in the vegetable world, in short, for imitating vegetable products. Since many of the excretions of the animal body contain tolerably complex atoms of

organic matter, the statement is only in part true, that the animal gives off to the external world carbonic acid, water, and ammonia; even if we should regard the urine as representing an ammoniacal salt, and if we considered the taurine of the excrements and the formic, butyric, acetic, and caproic acids of the sweat, as equivalent in this view to carbonic acid and water, we still could not deny that both men and the lower animals daily give off no inconsiderable quantity of protein-bodies directly to the external world, since the solid excrements are never free from mucus, and since the epithelial desquamation and waste of other horny tissues are sufficiently great to admit of calculation. . . . . . We have already remarked that in plants there are several processes of oxidation proceeding simultaneously with the general de-oxidation; so also in animal life, besides the oxidation in the blood of the capillaries, we perceive a number of reducing processes, which, in their intensity, hardly yield to the corresponding processes in plants; thus we have seen that substances whose reduction requires the most powerful agents,—as, for instance, the sulphates,—are deprived in the *primæ viæ* of their oxygen; and the peroxides of iron and mercury, and similar substances, are de-oxidized in the intestine; we have previously had occasion to mention that the fats and lipoids which are formed in the animal body can only be produced by a process of de-oxidation. Even if we should assume that oleic and margaric acids were produced from starch or sugar, by a process of cleavage (Spaltungsprocess) such as occurs when alcohol is formed from sugar, so that the reduction would be only apparent (as when a body separates into a highly oxygenous portion, as carbonic acid, and a portion poor in the oxygen, as alcohol, fousel oil, margarin, or olein); must not the stearic acid, which is only very rarely taken by animals as a constituent of vegetable food (for as yet it has only been found in cacao-butter), be formed by a direct process of de-oxidation, since its composition and its chemical qualities indicate that it can only be regarded as a lower stage of oxidation of the radical of margaric acid? And can we suppose that a substance which is so poor in oxygen as cholesterin, is formed by a simple disintegration of other organic matters? The oxidizing power of the animal organism is confined within tolerably narrow limits; sulphuret of potassium, when taken by an animal in not too small a quantity, passes in part unoxidized into the urine, through the highly oxygenated blood; salicine, in its passage through the blood, is not even converted into salicylic acid. Can the highly sulphurous cystine be produced by any other process than one of de-oxidation? When we consider the richness of many of the horny tissues in sulphur, and further, that they contain a group of atoms perfectly similar to albumen, we can hardly explain its formation, otherwise than by a local de-oxidation. If it be true that the iron is extracted from hæmatin by sulphuric acid and water, with a development of hydrogen gas, there must be a reducing apparatus somewhere in the animal body by which the iron which enters the organism with the vegetable food solely in an oxidized state, is deprived of its oxygen." (pp. 213—216.)

These illustrations are quite sufficient to show how impossible it is to erect chemical barriers between the organic processes in plants and animals: and we might increase them to almost any extent.

On reviewing the chemical substrata of the animal body, treated of in the first volume, we perceive that there are four groups of substances in which the vital processes are most intensely manifested; or, in other words, which most actively participate in the metamorphoses of the animal tissues. These are:

1. *The albuminous substances or protein-bodies, and their derivatives.*
2. *The fats.*
3. *The carbo-hydrates, and*
4. *The inorganic salts.*

That *albumen* is one of the most important substances in the animal

body is sufficiently obvious from the positions in which it occurs: we find it in the greatest quantity in the blood, and in all those animal juices which principally contribute to the nutrition of the organism; however, a chemical investigation of various tissues shows us that albumen only requires slight modifications to enter into other forms; as, for instance, that of syntonin or muscle-fibrin, the essential constituent of the solid contractile parts by which alone both the voluntary and involuntary movements of the animal body are accomplished. We find it both in its fluid and solid form in that most complex of all structures, the nervous system, both in the nerve-tubes and in their contents. In association with a little fat and traces of sugar, the ovum consists merely of albumen and casein holding salts in solution; and there can be no doubt that with the co-operation of the oxygen conveyed by the blood, all the tissues are formed from the protein-bodies, although we are not as yet in a position to explain with certainty the exact nature of the changes by which the gelatigenous and certain other structures are produced.

The fats next claim our attention. Their physiological value and their mode of origin have been noticed at some length in the first volume (see translation, vol. i. p. 248—272); we will here simply mention, that without the intervention of fat no colourless blood-cells, and therefore no red corpuscle, could be formed; indeed, no animal cell or fibre of any kind. While, however, in the normal state, the fat takes an active part in cell-formations in the animal body, we also, in some cases, perceived a tendency to a formation or production of fat in existing cells and tissues whose nutrition has been peculiarly modified. The phenomenon commonly designated as fatty degeneration admits of a double explanation. We may either assume that the pre-existing fat, under the influence of certain molecular forms, is accumulated in the older and less vitally active cells, and replaces the nitrogenous textural particles as they become worn out; or that the fat is produced directly from the nitrogenous textures of the cells or fibres, the nitrogen being developed in the form of ammoniacal salts, and the fat being left as a product of decomposition. Lehmann, in his first volume, supported the former view as the least hypothetical of the two, and as the more probable from its simplicity; since the date of its publication Wagner and Liebig have, independently of each other, instituted certain positive experiments which very much strengthen the second view. Wagner made the remarkable observation that crystalline lenses, pieces of dried albumen from eggs, and other substances poor in fat, which were introduced into the abdominal cavity of birds, were perfectly changed in their texture in the course of from twenty-five to fifty-four days, the residue containing far more fat than existed in the original substance. Liebig has shown that the metamorphosis of the albuminous tissues of the animal body into fat is, in a chemical point of view, not only possible but probable. Both in the putrefaction and in the gradual oxidation (by chemical means) of albuminous substance, the results, under favourable conditions, are ammonia and fatty acids, such as the butyric and the valerianic. These experiments and observations of Wagner's and Liebig's are, at all events, sufficient to show that there is no chemical absurdity in assuming that, under certain circumstances, fat may be a product of the decomposition of the protein-compounds.

The carbo-hydrates, constituting the third group, are in many respects closely allied to the fats. The substances of this class occurring in the animal body are, dextrine, milk-sugar, inosite, and glucose; to which, perhaps, we should add cellulose, occurring in the investments of the tunicata. Recent investigations have detected sugar, in small quantities, in almost all the fluids subservient to nutrition, as, for instance, in the blood. transudations, lymph, chyle, the albumen of the egg, &c.

"The sugar," says Lehmann, "which we meet with in the intestinal canal of herbivora and omnivora is due to the metamorphosing influence of saliva and pancreatic juice on starch and other carbo-hydrates; but we also find sugar in the blood of carnivorous animals in no very inconsiderable quantities; this must therefore have arisen from some other source than from the carbo-hydrates conveyed into the system from without; from a number of comparative analyses of the blood of the portal and of the hepatic veins, I believe, that I have indicated the probability that the sugar which is found in the liver, where it has also been found by Bernard and Frerichs, owes its origin to the decomposition of albuminates, and especially of fibrin."

When, further, we consider that nature has provided the egg with a small quantity of sugar, and that its amount varies according to the stage of development of the chick, the conviction forces itself upon us that the sugar, like the fats, is intended for some other purpose in the economy than merely to sustain the animal heat by its slow oxidation.

We are still far from being fully acquainted with the carbo-hydrates and the products of their conversion, which occur in the animal juices. Lehmann believes that we shall find indifferent substances similar to Scherer's inosite in the extractive matter. Our knowledge is more perfect regarding the acids which are formed in the animal body from the carbo-hydrates: formic and acetic acids have recently been found by Schottein, a very promising young chemist, in large quantities, in the sweat; butyric acid occurs, not only in the sweat, but also in the muscular fluid, in the parenchymatous juice of the unstriped muscular tissue of the stomach, the intestinal canal, and the urinary bladder; and lactic acid is found in the gastric juice as well as in most of the above-named fluids.

We regret that we cannot follow our author through his demonstration of the facts that the presence of small quantities of sugar essentially contributes to the solution and digestion of the protein-bodies, and that the carbo-hydrates, or rather their acid products of metamorphosis, discharge an important function in the intestinal canal, in no way directly connected with the process of respiration.

The most interesting portion of the volume still lies before us, and we hope in an early number to resume our analysis of it.

G. E. Day.

Review VII.

Traité Pratique des Maladies Cancéreuses et des Affections Curables confondues avec le Cancer. Par H. Lebert, M.D., &c. &c.

Practical Treatise on Cancerous Diseases and on Curable Affections confounded with Cancer. By H. Lebert, M.D. &c. &c.—*Paris*, 1851. 8vo, pp. 892.

M. Lebert, well known for a considerable number of years as an ardent cultivator of microscopical histology, has here produced a book of high pretensions and solid worth on one of his favourite themes—Cancer. The ideas of M. Lebert on the intimate structure of this morbid product have been so widely promulgated, through the medium of papers in the French and German periodicals, that our readers must not expect much of actual novelty on this branch of the subject. Still notions, now familiar, are presented in new aspects,—arguments more or less novel are found in their support,—recent objections from various quarters are more or less ingeniously met; so that even in the department of histology an analysis of this volume may not prove valueless, even to those well acquainted with the peculiarity of M. Lebert's cell-doctrines.

The volume is written on a plan sufficiently common now in managing the history of diathetic diseases. In a first part, the pathology of cancer in general is considered; in a second, the disease is described as it occurs in particular structures and organs.

The first part opens with a 'definition' of cancer (in point of fact, and, in logical phrase, merely a brief 'description'), the prominent feature of which is, of course, the specific character of the microscopic elements of the product. The 'definition' is otherwise distinguished by the broad intimation it gives of the author's incredulity as to the cure of cancer being ever effected by surgical extirpation, by its agreement with the doctrine (for several years past taught in this country) which rejects, as unsound, the antiquated division of tumours into the classes 'benignant' and 'malignant,' and by its emphatic expression of the invariably fatal issue of the disease in every instance, where the author has had an opportunity of "following its complete evolution."

Turning to the naked-eye characters of cancer, we find M. Lebert insisting on the importance of its peculiar milky juice; this he holds to be an attribute "almost as constant and characteristic as the presence of the cancer-cell." In the significance assigned to this juice, and without the qualification appended by M. Lebert, British pathologists are probably prepared very much to acquiesce. We have long held this fluid to be *almost* an unfailing test of the cancerous or non-cancerous nature of a tumour; but unfortunately there are instances, rare though they be, in which a whitish fluid, not distinguishable with the unassisted eye from that of cancer, may be expressed from fibrous and simple exudation-tumours. It is, as a single character, the least likely to deceive, but it may deceive.

M. Lebert yields somewhat of his original confidence on the point in admitting that it is not always possible to determine, from the characters

of a given *isolated cell*, whether it belonged to a cancerous growth or not. His affirmation now runs thus: from the microscopical examination of any given "morbid tissue," its cancerous or non-cancerous nature may always be established. The type of the cancer-cell is a "small regular sphere, with an elliptical nucleus, placed excentrically, filling nearly the half, or upwards, of the interior of the cell, and containing one or more large nucleoli." But this typical form is often not maintained with purity; the cell-wall assumes an ovoid, elongated, or triangular shape, with acute or obtuse angles. According to M. Lebert it may even be of true 'fusiform' outline—a shape we have never seen; all varieties of 'shapelessly caudate' form, we agree with him in recognising as of occasional, even tolerably frequent, occurrence. To this multiplicity of contour much importance, and with good reason, is attached by the author; and he fairly urges that if similar diversity may be found, on superficial examination, in epidermic tumours, it can always with ease be traced, either to folding or some morbid change of the epidermic cell.

M. Lebert holds to the reality of his so-called "concentric cell:" he has, of late years, frequently met with it in the testicle, lung, mamma, and omentum. "In some of these cells a perfect cell-wall was surrounded by another cell-wall, as regular as, and larger than, itself; in some the concentric character consisted in the superposition of several membranous involucra of lamellar appearance. Few persons have been fortunate enough to see these cells. Henle disrespectfully suggests, that M. Lebert describes them from imaginary forms, or from starch-cells—a suggestion naturally repudiated with indignation by the accused.

The following passage contains so important an admission, that we extract it in full.

"There are circumstances in which microscopical examination does not display the typical characters of the cell in a cancerous tumour. But, in the first place, the occurrence is so exceptional (only in from two to three per cent. of cases) as not to invalidate the general rule. These exceptional tumours contain imperfectly developed cells, in which the cell-wall is commonly wanting; while the nuclei of small size, from 0·005 to 0·006^{mm}, only contain, in rare instances, a characteristic nucleolus. These incompletely developed cells have been chiefly found in very soft cancerous tumours, developed with great rapidity. We have three times observed this condition of things in encephaloid, developed in the interior of bones; and have also established it in several cases of very rapid and very extensive secondary dissemination. As a general statement, it may be affirmed, that while encephaloid of medium consistence, and not over-rapid growth, is the cancer exhibiting the richest variety of cells, so, on the other hand, does it excite the most abundant cell-formation—cell-formation, under these circumstances, qualitatively the most imperfect."

The significance of this paragraph is clinically immense. Here it is formally admitted, that the most active and baneful cancer may vegetate through the system without exhibiting any of the cells we are elsewhere taught to regard as habitually essential to its existence. Here is ample corroboration of the justness of a proposition printed some years ago by ourselves, and which we may perhaps be pardoned for referring to, on account of the importance of the question at stake:—"A tumour may present to the naked eye the characters of encephaloid, be the seat of interstitial hæmorrhage, affect the communicating lymphatic glands, run in all respects the

course of cancer, and nevertheless contain no cells but such as are undistinguishable, in the present state of knowledge, from common exudation-cells."* M. Lebert places himself in this dilemma: either cancer exists without the cancer-cell, or simple exudation-matter produces the clinical manifestations of cancer. This at least is the straightforward practical admission, to which we are forced by the facts he admits; that the first of the terms of the dilemma is the one to be accepted, however paradoxical this may seem, cannot be questioned. But we are prepared to concede, as the above extract shows, that with the advance of knowledge, differences will probably be distinguished of a positive kind between simple exudation-cells and those of similar physical aspect, yet dynamically cancerous. Increased experience will probably teach pathologists to distinguish with surety cancerous-cells in unusual conditions of development from the exudation-cells they resemble. In the micro-chemical qualities of different varieties will very possibly be found a guide to their distinction.

Having described the various morbid changes which cancer-cells undergo, (diffluence, thickening of the cell-wall, diffusion, granular and fatty infiltration, and desiccation), M. Lebert turns to the discussion of the objections urged in different quarters to the doctrine of the speciality of the cancer-cell. Unfortunately the argumentation is long, and will not bear condensation. We unquestionably think it triumphant in certain points of view; it shows, as far as reasoning broadly based on observation can, that in *philosophical pathology*, the special cancer-cell is a real entity; but it fails to show (it could not show, if what we said a moment since be true), that in *practical medicine* the nature of growths can always be determined in the present state of histology, by the characters of their cell. We subscribe to the position, that the main source of disbelief in the speciality of cells has been misinterpretation of the various forms they assume in varying phases of development, and under the influence of disease; and we hold absolutely with the author, that the possibility of the direct conversion of the elemental forms of one kind of structure into those of another (as of an epithelial or a liver-cell into a cancer-cell), is negatived by all unbiassed observation.

M. Lebert has performed some very careful injections of cancerous growths with the view of settling the nature of their vessels. Several years since M. Berard made the curious announcement, that these tumours were well provided with arteries, but wanting in veins. Although this statement was based on the results of actual injection, it appeared so inconceivable that blood should be brought to a texture unprovided with the means of returning it, that the opinion of the Paris professor had few converts. M. Lebert, employing yellow injection for the arteries and blue for the veins, has found abundance of vessels of both classes, though in variable proportions, with an intervening capillary rete, tinged green by the union of the two colouring materials. Like his predecessors, the author has failed in finding either lymphatic vessels or nerves in cancer.

To the chemistry of cancer M. Lebert offers no contribution. It is needless almost to remind our readers, that all the analyses hitherto published fail to throw any light on the nature of the morbid product: they are valueless, too, in point of diagnosis: many of the vaunted analyses of

* Cyclopædia of Anatomy, art. "Products, Adventitious," p. 138.

tubercle differ as much from each other, as they do from those of cancer. It is a striking fact, that, as far as we know, analyses of such compound products are never the work of men holding high reputation in their special science,—a pretty significant hint that they regard such attempts, in the present unsettled state of organic chemistry, as at the least bootless, if not positively unphilosophical.

M. Lebert very justly raises his voice against the doctrine of the 'degeneration' of non-cancerous tumours into cancers. The notion, indeed, one of the most flagrant heresies in pathology, could not survive an hour, did it not furnish so convenient a loophole of escape from errors in diagnosis. A given tumour is pronounced by experienced persons, at an early stage of its growth, to be non-cancerous. It enlarges, and is cut out. The qualities of cancer are recognised. But no error, it is held, has been committed; the tumour was, in truth, of innocent nature at first; it subsequently 'degenerated.' Now, in point of fact, degeneration by conversion is an impossibility, and the only way in which the change from non-cancerous to cancerous character could be effected, is by the *de novo* development of cancer within the area, and from the vessels, of a non-cancerous formation. Experience proves, that while there is no *à priori* impossibility of its occurrence, such development is infinitely rare; for our own parts, years ago we wrote that we had searched in vain for an example of it in fibrous tumours, its alleged most common seat, nor have we succeeded since. M. Lebert states that he has twice known cancer form in tumours, primarily non-cancerous, but does not mention their nature. It must, then, be admitted, that in excessively rare cases a change simulating 'degeneration' to the merely practical eye, does actually occur; but this is a very different thing from real degeneration being a common phasis of evolution of non-cancerous growths.

The author agrees fully with many previous writers in regarding cancer as a blood-disease, general from the first, no matter how purely local the malady may appear in its manifestations. The mode of origin of the solid product by cell-germination from a fluid blastema, is described as by writers in this country. But we think M. Lebert disposed to grant too much, in not explicitly denying the possibility of the direct transformation of a coagulum of blood into cancerous matter. The appearances of such conversion seem otherwise explicable. Either *absorbed* blastemal elements of cancer are accidentally evolved in the interstices of a clot; or (an hypothesis supported by numerous facts) blastema, *not exuded from the vessels*, retained within them in fact, germinates in their interior.

Admitting that he has "not been able to study the mechanism of the vascularization of cancer," the author nevertheless ventures to treat as "positively false" the opinion, "that the vessels of cancer form in its interior, independently of the general circulation." We are not aware that any one has professed that the vessels of cancer are *solely* formed on the *de novo* plan. But the opinion that they are in part thus formed, seems warranted, by the analogy of the process in the vascular area of the chick; by the fact that the new cannot at first be completely injected from the old vessels; by the analogical truth, that new blood-particles appearing in lymph in the frog are of spherical shape (as in the fœtal

condition), and are therefore not particles previously contained (for these are oval) in the old vessels. Besides, direct observation with the naked eye, pocket-lenses, and the microscope, seems to show that there are some independent vessels. M. Lebert simply affirms that "the vascularity comes from the general circulation from the first." But how does he know this? And granting that he has abundantly injected cancer-vessels from those of the contiguous textures (which no one questions), where is his proof that he can thus inject them *all*? and granting that he does inject them all, where is his proof that the connexion of some of those vessels with the original circulation was not effected by *secondary* inoculation?

How do cancerous tumours enlarge? Of course, essentially by the contribution of new blastema from the blood. But, in assimilating that blastema, do, or do not, already formed cells play an important part? Is the procreative or vegetative faculty of cells a reality or a fable? Is the procreation of each new series of cells wholly independent of those that have gone before? or are the former produced, in part at least, from or through the latter by endogenesis or exogenesis? M. Lebert repudiates utterly the notion of the procreative faculty of cells in general, and cancer-cells in particular. He lavishes light banter and heavy indignation on those who imagine they have "caught nature in the fact, and stood by at the parturition of a mother-cell, and birth of its young." But, except the statements that M. Lebert has studied animal cells much, both before and since the advent of Schwann, and that he is "perfectly convinced" of the non-existence of cell-multiplication, we find nothing tendered bearing even the semblance of evidence that the cell-accoucheurs aforesaid (as the author, in pursuance of his metaphor, would doubtless call them) are wrong in the principle they advocate. M. Lebert should take to pieces, *seriatim*, the facts and arguments of those who maintain that the cells of certain growths are vegetative and evanescent, if he sincerely hope to undermine their belief.

M. Lebert deposes strongly against the importance frequently attached to the characters of cancerous ulcers, as diagnostic of their nature. The hard and callous, thick and everted edges, &c., he justly observes, may be seen in old ulcers of all sorts. This is, unfortunately, too true; were it otherwise, the distinction of cancroid and syphilitic from cancerous ulcers would not be so difficult as it is. The result of the author's observations in general is, that "in several organs cancer ulcerates either very rarely or not at all, and that in the organs where it ulcerates most frequently, it does not undergo this change in a quarter, a third, or half of the cases."

We find described, under the title 'phymatoid,' cancerous substance rendered tuberculous in naked-eye appearance by infiltration with fat; but except in the invention of this word, there is nothing new in the paragraph or the subject. That cancer is given this appearance in the manner mentioned, has long been generally known. Neither in the arguments here given in refutation of the opinion of Bochdalek, that the local cure of hepatic cancer by fatty conversion is of pretty frequent occurrence, do we find anything novel. In his refusal to receive that opinion we wholly agree with the author. This fatty aspect occasionally presents itself distinctly in portions of tumours which are rapidly growing and infiltrating

the adjoining tissues with characteristic creamy juice at the very time of its discovery; so much so, that the idea sometimes suggests itself, whether the very destruction, by fat-infiltration, in one part of a tumour may not give a stimulus to its growth elsewhere.

Authors in general admit that cancer destroys life in various ways. The writer of the present volume professes a different doctrine. He has met with such a number of cases of deaths from the progress of a *single* cancer (unattended with secondary formations) "in which there was neither abundant suppuration, nor repeated hæmorrhages, nor serious functional disturbances, to cause death," that he has "come to the conviction that cancer kills, after a certain duration, by *general death of the system*, and not by a purely local affection—by *infection of the entire economy*, and not by incidental disturbance of an important organ. Cancer kills by depriving the blood of the power of sustaining life, not by causing loss of blood either unchanged or altered through exudation." That the condition of blood existing in cancer is capable of killing seems certain; but M. Lebert's notions would lead us to attach too little importance to all morbid conditions beyond that fluid. Still, he argues his case pertinently:

"The reaction produced by secondary cancer, even when this is serious in character, is often so little apparent, that frequently, no matter what attention has been paid to the case, clinically during life, secondary formations are found in organs where no one expected their existence. Again, compare the bodies of two individuals, both cut off by a cancerous growth, which ran its natural course without disturbance, but the one presenting a single cancerous tumour only, the other numerous secondary formations, and you will certainly not find a great difference in the two bodies in respect of amount of discoloration, emaciati atrophy of the tissues, and impoverishment of the blood."

The importance of this doctrine in regard of treatment would be extreme, could M. Lebert afford us the material demonstration of the blood-alteration which he regards as so all-efficient. Here, however, he fails, as all who have gone before him. But who has proved alteration of blood, impregnated with syphilitic virus? And yet who doubts its existence? The day will come, however, and in all probability before long, when actual microscopical and chemical demonstration of the incipient alterations of the blood in diathetic diseases, as a class, will be obtainable, and so a material basis formed for the now prevalent conviction that there is a condition of the fluids capable of being therapeutically handled, which precedes local manifestations of the disease in each diathetic affection.*

The following table represents the amount of frequency with which primary cancer in the different tissues and organs named, was, in M. Lebert's experience, attended with secondary formations; the position of some of these tissues and organs in the scale, and the non-appearance of others, rather surprise us.

* In cases of Bright's disease, which we hold to be indubitably a blood-disease *ab initio*, the failure of treatment probably comes essentially of the existing inability to diagnosticate the affection, before its *local anatomical character in the kidney* has been developed. This anatomical character now absorbs attention; and to it treatment is directed almost alone. It would be just as rational to aim at the cure of phthisis, small-pox, typhoid fever, or scurvy, by local applications to the pulmonary tubercles, the cutaneous pustules, the intestinal deposit with its ulcerative effects, or the cutaneous blotches and spongy gums, which severally constitute anatomical characters of those affections.

Cancer of the	bones	0·77
"	liver	0·75
"	lymphatic glands	0·75
"	breast	0·71
"	kidneys	0·58
"	stomach	0·57
"	testicle	0·50
"	intestines	0·50
"	peritoneum	0·50
"	eye	0·45
"	uterus	0·33
"	brain	0·33

The general mean was 0·56.

In a subsequent section on "Cancroid," we find M. Lebert insisting on the importance of limiting that term to "the eating ulcer and to epidermic and epithelial productions, which are analogous to cancer in their mode of progress." Restricted thus in its signification, we see no serious objection to the term; for in very fact a cancroid is not pathologically a cancer, no matter how much it may resemble one in its clinical course, and it is right and proper to intimate the difference in essence by a distinction of terms. We have in this country long been aware how vain were the hopes held out by mere histologists, that cancroid was as profoundly different from cancer in vital effects as in intimate structure; we have long known how fallacious were the assurances tendered, that extirpation of the epidermic mass would prove an absolute cure. But it appears well, as M. Lebert was himself the original source of the erroneous opinions circulated concerning the clinical harmlessness of cancroid, to transfer from his own pages what we may call his recantation.

"The most baneful localizations of epidermic cancroid are in the lower lip and in the penis; while cancroid in these sites readily ulcerates, it is essentially hypertrophous in nature, and is especially destructive, in that the epidermic infiltration has a morbid tendency to diffusion and propagation, at first among the surrounding parts, and subsequently to the bones and muscles, in the case of the lip; to the corpora cavernosa in the case of the penis. Besides, it affects the adjacent lymphatic glands; those of the neck and below the jaw, in the instance of the lip; while, in the case of the penis, the glands of the groin become infiltrated with epidermis, and may break up and form actual putrid ulcers. Among the non-cutaneous sites of cancroid we may mention, as still more noxious, those of the tongue and of the neck of the uterus. Recurrence of the disease is frequently explicable in these cases by its tendency to propagation; so that we oftener meet with a continuation, as it were, of the original disease imperfectly extirpated, than a true relapse. That in these situations cancroid destroys life, is an incontestable fact. Hence there are points of relationship, but too real, between cancroid and cancer."

We repeat, all this teaches nothing of the smallest novelty to observers on this side the Channel; but, on the other hand, M. Lebert does teach us something new, if he be absolutely correct in the importance he assigns to certain differences in the pathological progress of the disease, as follows:

"In several situations cancroid is an affection of much less serious character, of slow progress, leaving the general health unimpaired, of much more limited localization, exhibiting a much less marked tendency to diffused propagation, and consequently holding out the possibility, or rather the certainty, of cure by complete

extirpation, and sometimes even promising a state of perfect innocuousness if not interfered with. Further, all our post-mortem examinations, in cases of cancroid, have invariably proved its strictly local nature, even in very noxious sites; we have never met with secondary deposits at any distance from parts in actual anatomical connexion with the original disease."

But it is as obvious as the day, that (with a single exception) every one of these qualities, conditions, or peculiarities are to be found in not a few, but in many, cases of true cancer. That exception, it is almost needless to observe, refers to "the *certainty*" of cure by complete extirpation. But, unfortunately for M. Lebert's argument, we may take leave to doubt whether any such fortunate result can be promised in the instance of cancroid, a whit more positively; with more probability, we admit, but not with more positiveness Return takes place after the extirpation of cancroid, with which a thick layer of healthy tissue—microscopically healthy—has been deliberately excised.

M. Lebert's notions of the mean duration of cancer in divers organs may be compared with those of other authors.

		Months.
Cancer of the	thyroid gland	6·50
„	kidneys	8·00
„	liver	9·00
„	ovaries	12·00
„	fauces	12·00
„	peritoneum	12·00
„	skin	12·00
„	bladder	12·50
„	œsophagus	13·00
„	stomach	13·00
„	respiratory organs	13·20
„	tongue	14·00
„	uterus	16·00
„	intestines	18·00
„	lymphatic glands	24·00
„	osseous system	27·12
„	eye	33·50
„	breast	42·00
„	testicle	42·00

The chapter on the ætiology of the disease leaves us in as complete ignorance of the causes of cancer, as it unfortunately found us. A point or two may, however, be touched upon. M. Lebert examined the question of hereditary transmission in 102 cases, and found evidence of such transmission in 14, or about one-seventh of the number. M. Leroy d'Etiolles formerly found only 1 in every 278 cases ascribed to hereditary influence by the practitioners of France. M. Leroy's informants had, however, in all probability, not investigated the point with the care bestowed on it by M. Lebert. And, even with the proportion of one-seventh of his cases deposing apparently in favour of hereditary transmission, M. Lebert is disposed to doubt its reality. Concerning the direct transmissibility of cancer by inoculation, the author philosophically doubts, although he has observed one fact strongly corroborative of Langenbeck's well-known statement on the subject. Some cancerous

juice (proved to be such by microscopical examination) was injected into the jugular vein of a dog; the animal died in a fortnight. The walls of the heart and the liver contained indubitable cancerous formations, varying in size from a pin's head to a small bean. But M. Lebert, knowing that cancer is common in dogs, is ready to admit that the animal may previously have been cancerous. At all events, he shrinks from drawing any positive conclusion from a single experiment.

Of 349 cancerous patients, 131 were men, 218 women; a proportion between the sexes very decidedly differing from that observed in this country, and one which we cannot help thinking must, from some accidental cause, lessen the contingent really furnished by the female sex in France.

The secondary phenomena, non-cancerous in character, which are observed in the victims of cancer, are carefully enumerated by M. Lebert; but there is a mixture, as will appear from the following catalogue, of changes which come really of the nature of the malady, and of those which originate in its mechanical influences, when occupying certain special sites. Thus, "spontaneous coagulation of the blood in the veins, dropsy in the limbs or serous cavities, excessive emaciation, discoloration of the skin, bloodlessness of the organs, fragility of the bones and muscles," give examples of the two kinds of secondary disturbance. A sub-inflammatory state of the alimentary canal is frequent, even in cases of cancer of other parts. Pneumonia is frequently a terminal affection—pultaceous stomatitis and obliteration of veins are also only observed towards the close. M. Lebert found recent tuberculization in 8·6 per 100 of his patients destroyed by cancer; but he has not observed the development of cancer in phthisical persons. As to the absolute excluding power exercised by either of these diatheses over the other we entertain grave doubts; that they rarely coexist in the same individual, especially both of them in an active state, is indubitable, as we long since numerically proved; but, on the other hand, we have in one instance, that of a youth aged about twenty, found encephaloid and tubercle *growing* together in the same lung. M. Lebert appears to us to attach too little importance to the difference of the mean age at which the two diatheses are most active, in explanation of their tendency to mutual exclusion.

The section on treatment pronounces a most unfavourable verdict on all methods of alleged medicinal cure of cancerous diseases. The author has no sympathy with those who, like ourselves, looking to the very considerable number of reputed cancerous tumours that have been cured by the persevering use—pushed almost to poisonous effects—of certain drugs, decline to condemn the statements of our predecessors as altogether erroneous, or altogether mendacious. To pass such a judgment unreservedly on the alleged experience of whole generations of medical men would be harsh, if the foundations of such judgment were solid and unassailable; while the proceeding would, it appears to us, be utterly unjustifiable on any actually attainable evidence. But M. Lebert, armed with the 'discoveries' of 'cancroid,' 'partial hypertrophy of the breast,' and 'fibro-plastic tumours,' denounces all who venture to believe it possible that perchance some real cancers were included in the multitude of putative cancerous tumours, of which past experience records the cure. He expresses himself as really pained that the writer of this article should

have lent himself to promulgate a "doubtful or rather a favourable opinion of the curative value of agents hitherto proposed." We think M. Lebert has scarcely acted justly here: in fairness he should have referred to our final estimate of the value of specific agents—an estimate conveyed in these words: "There is no medicine known having claims to the character of a specific in cancerous diseases, nor even endowed with the special attribute of invariably modifying the course of the affection. But this is no reason that such a medicine may not be found; the history of mercury and quinine teaches the folly of absolute scepticism in respect of the reality of specific agents." Now hear M. Lebert. "Although cancer proves incurable by the agents hitherto employed for the purpose, we need not by any means despair of one day finding an agent capable of curing it. The action of bark in intermittent fever, and that of mercury against the syphilitic virus, when become constitutional, bear unmistakable witness to the fact, that there exist in nature agents capable of neutralizing the action of a deleterious morbid principle in the system; and logical induction certainly warrants the hope, that the fortunate day of the discovery of a specific remedy against the cancerous principle will come." What is the difference in the doctrines professed in these passages? In the very illustrations employed, they are almost identical. What is the last-quoted passage but a paraphrase of the first?

Turning to the "surgical treatment of the affection," we find M. Lebert regretting the existing deficiency of satisfactory statistical returns, and especially tearing to pieces the oft-quoted series of results obtained by M. Leroy from various practitioners of France. That accurate statistical returns are wanting, all the world will agree in admitting, with M. Lebert. But in his pleas for the operation ("solely as a palliative, as a means of lessening suffering and prolonging life") he forgets that the very imperfections of M. Leroy's information—the errors of diagnosis by which his estimates are assumed to be utterly vitiated—give additional force to the opinion of those who regard excision, with whatever intent performed, as detrimental in the mass of cases. For it is a starting-point common to those who recommend as a rule, and to those who oppose as a rule, extirpation with the knife, that the chances of beneficial results are vastly greater with non-cancerous than with cancerous tumours. If, then, M. Leroy, or any other person, obtains results positively unfavourable to operation (either as a means of curing the disease or of prolonging life), from the analysis of a group of cases professedly cancerous, but among which non-cancerous growths figure to a considerable amount, it is self-evident that those results would be still more unfavourable, if accurate diagnosis had excluded all non-cancerous products. The greater the number of instances of confusion of fibrous, sarcomatous, and simply hypertrophous masses, the greater the gain to those who affirm, the greater the loss to those who deny, the efficiency of operation. M. Lebert and others, who set great store by the "error of diagnosis" argument, and seem to imagine that it annihilates their opponents, in point of fact simply expose the weakness of their cause by dragging it to light. Why, too, does M. Lebert ignore the telling experience of Scarpa, of Benedict, and Macfarlane? But, truth to say, it matters little which side he espouses, and what experience he selects for its defence; for, as a *curative* measure, excision has

been tried, found wanting, and condemned by all but the universal voice; while, that it succeeds "occasionally" as a *palliative* measure, no one, on the other hand, denies. The only function which statistics could now perform in the matter would be to convert the term "occasionally" into so much per cent.; to give us the per-cent. frequency, of palliation. And at the bed-side that crude numerical return, if obtained, would, unfortunately, serve us but little. What answer would it supply—perfect though it were in its way—to the momentous question: Here, in this particular case before us, of A or B, is it better to use or reject the knife? None, absolutely none. That rough per-cent. estimate would not help us an iota in placing them in the category of operable or non-operable patients. True, there are rules given in all quarters for guidance in this difficulty. But, even in M. Lebert's apprehension, these rules are of paltry clinical value. "Who," he observes, "has not seen numerous cases of operation in which relapse did not take place for some years, although the early progress of the case had been rapid, and of ominous character? Nor is it uncommon to find, that of two patients operated on, the one for a cancer of slow progress, the other for one of more rapid development, the latter (in apparently the worse condition) has no return of the disease for a much longer period than the former." The author himself adds no rules for guidance in this matter to those already printed by various of his predecessors. It would appear that the only chance of obtaining trustworthy data to aid us in determining whether the operation be opportune or not, is by *well-digested* numerical returns—that is, let a large number of cases of operation, thoroughly well observed in respect of the local disease and the constitutional state, in a word, of all clinical conditions—cases in which, too, the period of return of the disease is known with precision—be carefully tabulated. The analysis of the table would, in all probability, detect certain circumstances common to cases of early return, certain others, both positive and negative, uniformly occurring in those where operation had secured a protracted release from the complaint. At least, if the basis of a tolerably sure judgment as to the issue of excision be not supplied in this way, we are without hope for the future; the *à priori* and the *mixed à priori and small-experience* plans have lamentably failed. The man who would devote some years of his life to the collection and analysis of a mass of such cases, all of them either under his own care, or at least observed by himself throughout, would deserve well of his kind.

We have reached the second part of the volume,—that descriptive of cancer in particular tissues and organs. The chapter on the Breast arrests our attention, although this is precisely one of the organs on which it is most difficult to say or devise anything new. Under the title of "elastic cancerous tumours" M. Lebert describes growths intermediate between scirrhus and encephaloid, and forming about one-fifth of the total number of mammary cancers. They are composed of a pale yellow "tissue," homogeneous, elastic, and shining, exhibiting under a common lens a delicate, whitish, fibrous stroma; opaque granulations are also visible in them. The cancerous juice exhibits its usual characters, and in several tumours encephaloid, scirrhus, and this intermediate substance, coexist.

The writer gives an interesting table of thirty-four post-mortem examinations of cases of cancer of the breast, showing the frequency of secondary deposits in cancer of this site.

	Secondary Cancers.		No Secondary Cancers.		Totals.
Females dying without operation	5	...	1	...	6
Females or males dying a certain time after operation	8	...	2	...	10
Females or males dying of the operation	5	...	6	...	11
Insufficient clinical particulars	6	...	1	...	7
	24	...	10	...	34

Hence it appears that secondary cancers had formed in about five-sevenths of the cases. It would further follow, from this table, that the immediate and direct mortality from the operation is something enormous,—one in three, or eleven in thirty-four. This inference is unfair to the operation, however, for five of those fatal cases given in the table, are furnished by a friend; and the author knows not what number of operations may have supplied the five deaths. M. Lebert's own numbers on this point are 6 and 34; 6 deaths immediately in 34 operations, or about 18 per 100. Even this is a most serious contingent of mortality produced by the knife—more serious much than any we have found recorded by others. Of 130 operations (Benedict and Macfarlane), 4 were immediately fatal; "of 25 patients operated on, 2 died from the effects of the operation," according to the late Professor Reid's tables of the practice in the Royal Infirmary of Edinburgh (1825).

M. Virchow, whose name generally appears in association with sound doctrine, has broached a singular opinion on retraction of the nipple,—which, if well founded, would oblige practical surgeons to forget some of the notions they have regarded as most thoroughly established. He holds that "drawing in of the nipple" is assimilable to the contraction of cicatrices, and indicates a curative tendency! M. Lebert disagrees wholly with his "learned friend" on this point, and takes occasion to give the analysis of 27 cases in which adhesion and retraction of the nipple existed: 1st, 10 cases in which the cancer had only lasted from 6 to 15 months, and was making incessant progress; 2ndly, 10 cases in which the adhesion was accompanied with redness on the surface, and all the signs which precede ulceration; 3rdly, in 2 cases the retraction existed in a breast, secondarily affected after the other; a case in which death occurred after the gradual increase of the second cancer; 4thly, in 1 case the retraction was chiefly caused by the development of a cyst in the vicinity of the nipple; 5thly, in 2 patients retraction of the nipple was observed, though the progress of the case was very slow, and of "apparently benignant character;" 6thly, in 2 cases the conditions of retraction could not be determined. From all this it follows, that there are certain rare cases in which retraction of the nipple has no signification either *pro* or *con*, in regard of the serious character of the disease; but not a single instance appears lending support to the paradox of Virchow.

The facts analyzed by the author in reference to the progress and duration of mammary cancer, and the inferences deduced from them, are of such precision, that we extract the substance of his observations on this matter.

"We possess data on the progress and duration of 59 well-observed cases of cancer of the breast: in 27 of the number the duration was ascertained from outset to death; in the remaining 32 it was only known up to the time the

patients were lost sight of. But as in the greater number of instances these latter patients had nearly reached the fatal turn, and as they had, for the most part, undergone one, two, or several operations, their cases may also serve us for the statistics of the operation.

"The first point to determine is the time the patients presented themselves to undergo the first operation, and, consequently, the duration of the disease at that moment. We possess information on this point in 34 cases; in about five-sevenths of these the duration had ranged between three months and two years,—in two-sevenths the duration had been greater. Two patients must have been operated on before the end of the third month; but in these cases, as well as in all those at present under consideration, it must be borne in mind that we could only fix the outset at the time of manifestation of tumour, which is some months posterior to the real time of its first formation. Eight patients were first operated on from three to six months after the outset; 12 (6 of them in each three months of the period) between six months and a year; 5 between the twelfth and twenty-fourth month; 4 between two and four years; 3 between four and ten years.

"If we next consider the relapses after a first operation, and the interval of time between the first and second operation, (a matter in which we have 21 cases for our guidance), we learn that the enormous proportion of two fifths of the second operations were required in from between one to three months after the first,—and of course the time of relapse commencing must have been earlier still. Six of the remaining patients underwent the second operation from three to six months after the first; 4 at the end of one or two years; 2 at the end of from two to three years; and 1 at the end of eight years. If from the 34 patients operated on, we subtract the 6 immediately cut off by the operation, we have 28 remaining,—of which 28 there are 21 in whose cases we have positive information concerning relapse,— and this relapse occurred twice ... of thrice within six months,—and in somewhat less than a quarter of the cases in from one to three years. We should be glad to give the remaining 7 cases as examples of cure,—but the probability is small that they were so. They are cases of patients who left the hospital, recovered from a first operation, and who were lost sight of But nothing justifies us in supposing their fate a better one than that of patients whose subsequent career was watched, and who all had relapses.

"We must, however, here make a qualification favourable to the operation,—to which we are not absolutely hostile under all circumstances. We have, in truth, in some rare cases, seen relapse not take place until some years after a first operation, required at the end of six to nine months after the first manifestation of the tumour,—whereas we have, on the contrary, seen very prompt relapse in patients who had had cancerous tumours in the breast for several years.

"We have also notes, in five cases, of a third operation performed after a second relapse,—three times in the space of from one to three months after the second operation, once in the space of from three to six months, and once between six and nine months Among the 6 cases of death from the immediate effects of the operation, there are 2 in which it was performed in from three to six months from the outset of the disease, and 4 in which the complaint had existed from one to three years. . . . .

"We reach a second order of cases, 19 in number, of patients not operated on, and of whom 6 are still living, 13 are dead. Among the 6 survivors are 2 in whom the disease has existed from one to two years, 1 from two to three years, 2 from four to six years, and 1 for twelve years. Among the 13 who are dead, the duration was in 1 case from three to six months, in 6 from one to two years, in 3 from three to four years, in 1 case twelve years."

It is unnecessary to enter into any elaborate commentary on these facts; they speak for themselves. True, there is nothing new in the results; they

simply corroborate the inferences deduced from observation by men of all times and all climes concerning the inefficiency of operation. But at all events, the eternal objection of "error in diagnosis" (an objection which we have, however, already shown has no real force) cannot be raised against these results by the microscopical school.

The history of mammary cancer is followed by that of "partial hypertrophy of the mammary gland," none other than our old acquaintance, the "chronic mammary tumour" of Astley Cooper. To this section we turned with real interest and curiosity; for as M. Lebert denounces repeatedly, and in no gentle terms, those who confound this disease with incipient scirrhus, we felt assured that rules for effectual distinction of the two diseases would be furnished in number. What was our disappointment to find the clinical aspects of the subject left by M. Lebert exactly as he found them! He who stands puzzled before a breast, doubting whether the nodule he feels is a simple tumour inclining eventually to become cystic, or a very cancer, will not find a syllable to release him from his dilemma in the pages before us. And it is downright absurd to insist, as the author does, on the preservation of the general health as appertaining to the chronic mammary tumour, when, as he admits himself, at the early period of scirrhus (and then alone is the diagnosis difficult) the general health may be excellent—nay, often actually is above par. It is true, M. Lebert gives a very elaborate description of the anatomy of the disease—a description which, if in all points correct (and this we have no reason to doubt), justifies the new name he has given it—and shows that its intimate nature is in some respects unlike the previous conceptions formed of it. But clinically this is valueless. The only anatomical character having important clinical bearing, and of which the author makes much, is the tendency to cyst formation—a tendency well known to all who have glanced even at the writings of Sir B. Brodie.

We regret to be forced, though not by deficiency of matter, to throw down our pen here. There is yet much in the volume which will repay patient study, and to which we have not been able to make even a passing allusion. Indeed, the present article has no pretensions to the character of an analytical review—being, in truth, little more than a notice of some of the prominent points in the work. Of that work the main characters are fulness and precision of detail, careful avoidance of hypothesis, and sound general views in pathology and therapeutics—characters, it will be admitted, likely to attract a numerous corps of readers to its pages.

W. H. Walshe.

Review VIII.

1. *Présence de Sucre dans les Urines.*
On the Presence of Sugar in Urine. By ALVARO REYNOSO.—('Comptes Rendus,' t. xxxiii.—xxxiv.)
2. *Absence de Sucre dans les Urines de Personnes Hystériques et Epileptiques, quelques heures après les attaques.* (Note de M. MICHÉA.)
Absence of Sugar in the Urine of Hysterical and Epileptic Persons after the Fits. 'Comptes Rendus,' t. xxxiii.
3. *On the Presence of Sugar in the Urine of Aged Persons.* By A. DECHAMBRE. ('Gazette Medicale,' No. 14, 1852.)

SINCE the interesting researches of Bernard were made known to us, the subject of sugar in the urine has occupied the attention of physiologists both in this country and on the continent. After the experiments of Bernard on the effect of irritating the floor of the fourth ventricle in rabbits and other animals had been repeated, and the startling results obtained by him confirmed by numerous observers, it became an important point to establish a theory which would satisfactorily afford an explanation of the remarkable phenomenon which had been recorded. With this view experiments were repeated and varied according to the notions which presented themselves to the minds of different experimenters, until a vast number of new facts were obtained, which opened new fields for inquiry in various directions, but all bearing more or less on the subject of the presence of sugar in certain secretions, and also in the circulating fluid from which these secretions were elaborated.

Bernard repeated his experiments and extended his researches in those directions which he considered likely to afford new results, but up to the present time the only generalization which he appears to feel himself justified in offering as an explanation of the numerous and important facts he has discovered, is, that the presence of sugar in the urine, and of an undue amount in the systemic circulation, is due to increased activity of the function of the liver dependent upon an unusual excitation of the functions of the great sympathetic system of nerves. At present this is all he considers it right to advance. At the same time he continues the prosecution of his researches, but appears to wish to suspend his explanation of the origin of the changes until the investigations are more complete.

Other observers have attributed the presence of the sugar in the urine to the imperfect action of the respiratory functions, and consider that the results obtained by Bernard are to be explained by the respiratory changes being impaired in consequence of injury to the situation in which fibres of the vagus take their rise, rather than by the supposition that irritation in this part of the medulla stimulates the liver to secrete a greater quantity of sugar than in a perfectly healthy state, through the influence of the sympathetic system. If Bernard's view be correct, when sugar is detected in the urine it is a proof of increased activity in the sugar-secreting organ, more sugar being formed than is capable of being decomposed during its passage through the lungs, and in consequence the excess is eliminated from the circulating fluid by another channel. M. Alvaro Reynoso and others hold, on the contrary, that, instead of an excess, the normal quantity of sugar only may be formed, and that, in consequence of part of this escaping

combustion in the lungs, owing to the imperfect action of these organs, it passes into the systemic circulation, and is excreted by the kidneys. Bernard has advanced several weighty reasons against this view; the chief of which is, that after division of the vagi, although respiration is greatly embarrassed, no sugar appears in the urine.

M. Reynoso has lately put forward several statements in the 'Comptes Rendus' in favour of his view; he finds that in almost every case in which the activity of the respiratory changes is interfered with, sugar may be detected in the urine. In the first place, this observer states that he has succeeded in detecting sugar in the urine of persons subjected to etherization, and that of two men under the influence of the vapour of ether, the urine obtained from the most active will contain the greater amount of sugar (vol. xxxiii. p. 416.) Under other circumstances of incomplete asphyxia, it is affirmed that sugar passes off in the urine. Rabbits which had been suffocated or drowned furnished saccharine urine, and living animals which had been prevented from breathing freely became diabetic. M. Reynoso also cites in favour of his view the observation of Bernard, that sugar is to be detected in the urine of fœtuses.* The urine of persons under the influence of the bichloride and iodide of mercury, the salts of antimony, opium, and narcotics in general, or sulphate of quinine, was found to contain sugar. (p. 520.)

In the same volume (xxxiii. p. 606) of the 'Comptes Rendus,' M. Reynoso also states, that dogs which have been submitted to the action of arsenic, lead, or sulphate of iron, excrete sugar in the urine. He also detected sugar in the urine of persons treated with carbonate of iron, and in that of patients affected with tubercle, the sugar being more abundant in those cases in which the malady was in its most advanced stages, and when the inflammatory symptoms were most intense. In pleurisy, asthma, and chronic bronchitis, and in cases of hysteria and epilepsy, sugar was also discovered in the urine. The method employed by M. Reynoso for the detection of the sugar is illustrated in the following example:

"The urine of a healthy man was carefully tested for sugar, and he was then subjected to the influence of ether. About 100 grammes (1543·36 grains) of the urine passed after recovery, was treated with solution of diacetate of lead, in order to separate the lithic acid, &c., and the precipitate was collected upon a filter. The excess of diacetate of lead was decomposed by chloride of sodium, and the mixture again filtered The clear solution, after being concentrated by evaporation, was treated with the tartrate of potash and copper solution, or the yeast test was applied. Decided indications of the presence of sugar were obtained."

M. Michéa, in the same number, refers to the experiments of Reynoso, and also to the statements put forward in a work on Glucosuria, by Dr. Heller, in reference to the presence of sugar in the urine of patients afflicted with certain nervous disorders. The test employed by M. Michéa was similar to that recommended many years ago by Mr. Moore. Solution of potash was added to the urine, and the mixture was then boiled. If sugar were present, it should assume a dark orange-red colour after ebullition for a short time, and upon the subsequent addition of nitric acid, the solution

* M. Bernard examined the urine of the fœtuses of more than 150 cows and sheep, and he found it to contain sugar in every case. The tests employed were, the liquor potassæ, the tartrate of copper, and the fermentation tests.

would become paler, and at the same time the odour of melassic acid would be developed.

M. Michéa failed to obtain this reaction in the urine of four cases of hysteria, in which the urine passed four hours after the attack was subjected to examination. No indication of the presence of sugar could be obtained upon examining, in this way, the urine of seven cases of delirium tremens during the whole period of the malady; and a negative result was also obtained in six cases of general paralysis, five of mania, and three of partial delirium, in which the urine was examined each day during many weeks. In all—specimens of the urine of twenty-seven patients were subjected to examination by M. Michéa, and in not one could the least vestige of sugar be found.

We should here remark, that in a subsequent number of the 'Comptes Rendus' (vol. xxxiv. No. 1), M. Reynoso reiterates his former statements with reference to the presence of sugar in the urine of hysterical patients and epileptics, and offers to perform the experiments in the presence of M. Michéa.

In the 14th number of the 'Gazette Medicale' for 1852, is a report of some experiments by Dr. A. Déchambre, on the presence of sugar in the urine of aged persons. After mentioning Bernard's experiments and those of M. Reynoso, above referred to, Dr. Déchambre gives the result of six experiments on the urine of old people. The first subject chosen was an old woman of 81 years of age, whose respiration and heart's action were ascertained to be unimpaired by organic disease.

About 100 grammes of the morning urine were collected, and this quantity was treated with a solution of subacetate of lead, after filtration, the excess of lead salt was decomposed with a solution of carbonate of soda; after a second filtration the liquid was treated with the copper and potash solution of M. Barreswil, and an abundant reddish precipitate (suboxide of copper) took place after boiling for a minute.

The same experiment was performed upon the urine of five women whose ages varied from 68 to 81, one of whom had abscess of the arm, another rheumatic pains, the third a chronic affection of the skin, and the two others were suffering from contusions. The morning urine of each of these persons gave a characteristic precipitate when tested.

Next, specimens of the urine from eight healthy old women, of upwards of 70 years of age, with the exception of one who was only 63, were examined. In two cases only a slight yellowish cloud appeared, but in the remainder, a characteristic reddish precipitate was obtained. In a week's time the urine of seven of these cases was again subjected to examination. In two there was no trace of a yellowish cloud, in two others the cloud was slightly apparent, and in the three last cases a precipitate occurred.

The urine of four women from 70 to 92 years of age was examined, and was tested for sugar with the fermentation test. After being treated with acetate of lead and carbonate of soda as before, the solutions were concentrated, mixed with yeast, and placed in a retort, which was gently heated. By distillation at a gentle heat, about a gramme of a colourless liquid was obtained, which was boiled, and upon applying a light the vapour which was given off burnt with a bluish flame, leaving an unequivocal odour of alcohol. A similar experiment was tried upon specimens

of the urine of six more women of upwards of 70 years of age, with a similar result. In neither of these experiments was the carbonic acid collected.

In these experiments the quantity of sugar present was not proved to bear any relation to the state of decrepitude of the persons, for in some of the most healthy and most vigorous, sugar was detected in considerable quantity, while in others, extremely aged and very decrepit, only slight indications of its presence were obtained.

From the experiments just referred to, it appears that on one day there may be much sugar in the urine, while in that secreted on the following day, all traces of its presence may have disappeared.

M. Déchambre concludes, from these experiments, that the urine of old people, as a general rule, contains sugar.

The theory of M. Reynoso receives some slight support from an interesting observation of Bouchardat's, which was made about twelve months ago—namely, that in two slight cases of diabetes, the urine was secreted free from sugar upon causing the patients to inhale oxygen.

Dr. Bence Jones obtained evidence of the presence of sugar in the urine of a woman under the care of Mr. Cæsar Hawkins, who had been kept for twenty-four hours and more under the influence of chloroform; two ounces at least of chloroform were taken, and the first urine passed afterwards was examined by Dr. Bence Jones; by the ordinary method no traces of sugar were found, but by following Reynoso's directions, "slight but distinct evidence" of its presence was obtained.

Sugar has also been found in the urine in cases of gout and dyspepsia, by Prout; and in certain abdominal affections and hypochondriasis, by Budge. Lehmann detected its presence in the urine of a puerperal woman, in whom the secretion of milk was suddenly suspended on the fifth day after delivery.

We found indications of the presence of sugar in considerable quantity in the prune-juice expectoration of a patient suffering from pneumonia, which was secreted shortly before his death. Upon boiling a portion of the sputa with the tartrate of copper and potash test, much suboxide of copper was reduced, and fell as a pale brown precipitate. This experiment was originally suggested to us by Dr. Todd, and is mentioned in a paper on 'The Diminution of Chlorides in the Urine, or their Absence from that fluid in cases of Pneumonia,' published in the volume of the 'Medico-Chirurgical Transactions' for the present year (vol. xxxv.), where also a report of the case (case ix.) will be found. The urine of this same patient, which was probably secreted about the same time as the sputa, was of an acid reaction, and specific gravity 1018. It had a highly offensive odour, and upon boiling a portion with the tartrate of potash and copper test, not the slightest change indicating the presence of sugar occurred. This specimen of urine, however, was not removed until the post-mortem examination, which did not take place for forty-two hours after death, which may perhaps, in a measure, account for the absence of indications of sugar. In Bernard's experiments it was found, that although sugar might be detected in a liver removed from the body immediately after death, no indications of its presence could be obtained if the examination was not performed for some hours afterwards.

From the foregoing statements put forward by continental observers, we find it difficult to arrive at any decisive conclusion with respect to the presence of sugar in the urine, in certain cases in which the aëration of the blood may not be carried on with that degree of activity which is consistent with a condition of perfect health. The conclusions of M. Reynoso are, to a certain extent, confirmed by the experiments of Déchambre on the urine of aged persons, while they cannot be said to be in any degree refuted by those of M. Michéa.

This latter observer does not appear to have tested the urine according to the method which was employed by Reynoso, but has used the liquor potassæ test, with the subsequent addition of excess of nitric acid. Although objections may be urged against the employment of those tests which indicate the presence of sugar by the reduction of suboxide of copper (a subject which will presently be considered), the potash test is fraught with still greater objections. A specimen of urine will not often be found, which, when mixed with solution of potash, will not become somewhat darker by boiling, and we should scarcely expect to detect the odour of melassic acid when only very small quantities of sugar were present; moreover, upon the addition of nitric acid other odours are evolved, which, it appears to us, would prevent the recognition of the peculiar smell of melassic acid, even if it were present.

The observation of Dr. Bence Jones on the urine of a patient who had been kept twenty-four hours under the influence of chloroform, is interesting, as it shows that under these circumstances traces only of sugar may be present in the urine, while from the observations of M. Reynoso, we should be led to expect, in so favourable a case as the one referred to, indications of a large quantity; at the same time it must be borne in mind that the presence of sugar may not be constant.

When only small quantities or mere traces of sugar exist in urine, it is a matter of considerable difficulty to detect the presence of this substance with certainty; for whichever method is employed we are in danger of failing to obtain a decisive result. On the one hand we feel that small quantities of sugar may be present in urine, and we may altogether fail to detect a trace, while on the other we may obtain a reaction which might be pronounced to depend on sugar, when, upon the application of other tests, no indications of this substance can be obtained. Again; we find from the researches of M. Déchambre, that although sugar may be detected with certainty in the urine passed upon one day, no traces of its presence may be met with, if another specimen be examined in a short period of time afterwards; moreover, the occasional occurrence of a diabetic state, which may last for a few days, or even for weeks, and then pass off entirely, has been for some time well known to physicians. It is very probable that when traces of sugar only are present in a specimen of urine, the length of time which may have elapsed before it is subjected to examination may materially affect the results obtained upon the application of tests.

It is much to be regretted that M. Reynoso and Dr. Déchambre have not furnished quantitative analyses; for from the results which they have obtained we should be led to expect, that in some of the specimens of urine subjected to examination by them, there was quite sufficient sugar for quantitative examination.

We have obtained, as opportunities have offered, in order to multiply observations on this point, the urine from epileptics, from patients who have been placed under the influence of chloroform previous to undergoing surgical operations; and in some other cases the results of our observations will be found stated in the following cases. It will be observed, that in these observations, the reactions which occurred upon the application of the tests are only mentioned, and we have not ventured to say whether sugar were present or absent. Although the results cannot be looked upon as conclusive, either with respect to the presence or absence of sugar, they may, perhaps, have the effect of drawing the attention of other observers in this country to this very interesting subject.

In the following cases, the urine was tested, as in Reynoso's experiments, except that carbonate of soda was used instead of chloride of sodium.

A certain quantity of urine was treated with a solution of subacetate of lead, until a precipitate was no longer produced. The precipitate was then separated by filtration, and the excess of subacetate of lead neutralized by the addition of carbonate of soda. The mixture was again filtered, and the clear solution concentrated to a small bulk by evaporation over a water-bath. The tartrate of copper and potash test, and, in some cases, Trommer's, the fermentation, or Moore's tests, in addition, were applied to the concentrated solution. In every case in which chloroform had been taken, the first specimen of urine passed after recovery from its effects, was subjected to examination.

1. *Gasken;* removal of foot by Mr Fergusson. Under the influence of chloroform for 15 minutes. Urine clear, pale, containing a very slight flocculent deposit; reaction acid; sp. gr. 1008. 7000 grains were treated, as above described, with subacetate of lead and carbonate of soda. The concentrated solution was treated with the tartrate of copper and potash test. After the greenish mixture had been boiled for a minute, it assumed a light-brown colour, and after the boiling had been discontinued, it became opalescent. Upon standing, a flocculent precipitate collected at the bottom of the test tube. Another portion was boiled with an equal bulk of potash. It gradually darkened until it assumed a dark-sherry tint.

2. *Ann Dyer*, æt. 50; removal of breast by Mr. Bowman. Under the influence of chloroform for 10 minutes. Urine of a lemon-yellow colour; slight deposit; reaction acid; sp. gr. 1022. 700 grains, treated as above. The same character of change occurred, upon boiling with the tartrate of potash and copper test, as in case 1, but the reaction was not nearly so decided.

3. *Smith;* length of time under the influence of chloroform not mentioned. Urine of a yellowish-brown colour; reaction acid; sp. gr. 1024. 500 grains, treated as above directed. Upon boiling with the tartrate solution the change was not so decided as in case 1. Another portion was treated with Trommer's test in the usual way. The mixture became brown, but the yellowish-brown precipitate, which is characteristic of the presence of sugar, was not produced.

4. *F. S., an old lady, 87 years of age;* suffering from chronic bronchitis, accompanied with profuse expectoration; otherwise healthy; reaction acid; sp. gr. 1015. A little of the urine was tested with the tartrate solution. Upon boiling, the mixture became brown, but the only precipitate

produced was that of a little phosphate; this, however, was of a brown colour. 700 grains were treated, as in the other cases. Upon boiling the concentrated solution with the tartrate test, the greenish mixture became bright brown, and a flocculent precipitate subsided.

5. *J. W., an old man, aged* 96; suffering from a large sloughing ulcer on the left leg, otherwise in good health. Urine clear; no deposit; reaction acid; sp. gr. 1014. 1500 grains, treated as above. In this case the reaction was as decided as in case 4. It should be remarked, that in cases 4 and 5, the concentrated solution had a brown tint before the addition of the test. When the blue solution was added, the mixture assumed a greenish colour, which, upon boiling, became bright brown. In every case, unless stated otherwise, the mixture was raised to the boiling point, and as soon as decided ebullition had taken place, the lamp was removed.

6. *Charles Hicks, æt.* 9. *Epilepsy;* Urine passed after a fit, of a pale-yellow colour; reaction acid; sp. gr. 1019. 700 grains, treated as in the other cases. Upon boiling the concentrated solution with the tartrate test, the mixture became of a brown colour, and a flocculent precipitate subsided.

7. *Urine of a young, healthy man, æt.* 24. Clear, of a yellow colour; reaction acid; sp. gr. 1016. 1500 grains, treated as above. Upon boiling the concentrated solution with the tartrate solution, the mixture became of a brown colour, darker than occurred in case 6, but not so dark as in 1, 4, and 5. A flocculent precipitate subsided.

8. *Johanna Mead, æt.* 12. *Epilepsy;* generally has three or four fits a day, each lasting from five minutes to a quarter of an hour. The specimen of urine subjected to examination was passed about half an hour after a fit; reaction acid; sp. gr. 1016. 700 grains, treated as above directed. The concentrated solution, mixed with the tartrate test, assumed a greenish hue, which, upon boiling, became paler and opalescent, but no decidedly brown colour was produced.

9. *Urine from a boy, æt.* 8, the subject of an operation upon a cicatrix of the hand; under the influence of chloroform for 10 minutes. The specimen examined was passed within two hours after recovery; reaction acid; sp. gr. 1024. 600 grains, treated as above. Not the slightest change in the colour was produced by boiling a portion of the concentrated solution with the tartrate test for some minutes.

10. *Thompson;* operated on for cancer of the penis, by Mr. Fergusson; a quarter of an hour under the influence of chloroform. Urine contains much lithate of ammonia; reaction acid; sp. gr. 1020. 1000 grains, treated as in the other cases. Upon boiling the concentrated solution with the tartrate test, it assumed a light-brown colour. About a drachm of the urine was also tested in a similar manner, and a similar pale-brown colour was developed. Two drachms of the urine were mixed with yeast, and placed for several hours in a temperature of 80° to 100°, but not the slightest bubble of gas was formed.

11. *Ann Goring, æt.* 20; excision of knee-joint by Mr. Fergusson. The patient was under the influence of chloroform for about half an hour, and did not pass any urine for 18 hours after recovery, when upwards of a pint was voided; reaction very acid; sp. gr. 1030. Two drachms were fermented with six drops of yeast. A bubble of gas was formed which measured ·04 cubic inch, while in a mixture of corresponding quantities of

water and yeast, a bubble of gas not more than half the size was developed This experiment was repeated with the same result; 700 grains were treated with subacetate of lead and carbonate of soda, as above. The concentrated solution, when boiled with an equal bulk of the tartrate test, assumed a bright reddish-brown tint.

Of the specimens of urine from these cases, all but those of cases 8 and 9 became of a brown colour upon being boiled with the tartrate of potash and copper solution, and, in several, flocculent precipitates were produced, but these precipitates had not the peculiar appearance exhibited by that of the suboxide, which is decisive in favour of the presence of sugar. Of these nine specimens which became of a brown colour, five were from cases which had been subjected to the influence of chloroform, two were taken from aged people, one from a child suffering from epilepsy, and one from a young man in perfect health. Of the two remaining cases, one was a case of a child with epilepsy, and the other was that of a boy who had been placed under the influence of chloroform for ten minutes. In case 10 the presence of sugar was not confirmed by the yeast test, but in case 11, indications of sugar were obtained by this test. Is the change of colour from greenish to a brown or reddish-brown upon boiling with the tartrate of copper test, or with Trommer's test, to be taken as evidence of the presence of sugar? If so, sugar was present in the urine of a man, æt. 24, to all appearance in perfect health. If, on the other hand, the production of a precipitate of suboxide of copper is alone to be considered indicative of the presence of sugar, then there is not one of the foregoing instances in which we could assert that sugar was present, for, although in several a precipitate occurred, it differed materially in its character from that obtained by testing diluted diabetic urine, or a very weak aqueous solution of grape sugar in a similar manner.

It becomes necessary, then, to institute a strict inquiry into the degree of reliance which can be placed upon the tartrate of copper and potash solution as a test for small quantities or even traces of grape sugar in the urine, and we have therefore carefully performed the following experiments. The grape sugar employed was obtained from raisins, and by a previous experiment it was ascertained that a very dilute aqueous solution readily yielded the characteristic reaction with the tartrate solution. In many instances the results were confirmed by using a solution of pure crystals of grape sugar, obtained by acting upon starch with sulphuric acid; and corresponding results were obtained when diabetic urine was examined.

1. The precipitate of suboxide of copper was readily dissolved by acetic, hydrochloric, and nitric acids, and also by ammonia.

2. The precipitate was insoluble in a solution of chloride of sodium, but was readily dissolved by a weak solution of muriate of ammonia.

3. The addition of a few drops of a weak solution of muriate of ammonia previous to boiling, entirely prevented the precipitation of the suboxide, the mixture retaining its greenish colour. Upon adding some solution of potash, however, the precipitate was produced, and ammoniacal fumes were given off at the same time. If a moderate quantity of solution of muriate of ammonia was present, the precipitate did not occur upon the addition of potash, or by very prolonged boiling.

4. If a drop of a very dilute solution of the muriate was added to a pretty strong solution of sugar, and after the addition of the tartrate, the mixture was boiled, no precipitate took place, but the solution became of a pale brown tint; the suboxide being immediately thrown down upon the addition of a few drops of a solution of potash, with the development of ammoniacal fumes. In the above cases in which no precipitate took place, it was ascertained that there was excess of alkali present in the mixture.

5. A solution of oxalate of ammonia also prevented the precipitation of the suboxide, but a greater quantity of this salt was required.

6. A neutral solution of lithate of ammonia (artificially prepared) also prevented the reduction of the suboxide, and dissolved the precipitate if added to it. On carrying out this experiment, it was found that

The precipitate of suboxide of copper was dissolved by urine containing an excess of lithate of ammonia, as exemplified by the next observation.

7. A solution of grape sugar in water was prepared, and by a previous experiment it was ascertained that upon boiling with the tartrate test an abundant precipitation of suboxide occurred.

To a portion of the precipitate of suboxide produced in this way, about a drachm of healthy urine, immediately after it was passed, and while yet warm, was added, and the reddish precipitate was instantly dissolved, forming a perfectly clear solution. Upon further boiling, a slight precipitate took place (probably phosphate). The suboxide, however, could not be precipitated by the further addition of potash and prolonged boiling.

8. Upon mixing a small quantity of grape sugar with the same specimen of healthy urine, and boiling the mixture with the tartrate test, no precipitate, except that owing to the presence of phosphates, was produced. About half an ounce of the same mixture of urine and grape sugar was placed in a test tube, mixed with six drops of yeast, and inverted over mercury. The whole was then placed in a temperature varying from 70° to 100° for about twelve hours, at the end of which time the tube was found quite filled with gas, and all the liquid was expelled into the vessel in which it had been placed. The specimen of urine with which the above experiments were tried, was allowed to stand in a still place, and when it had become quite cold, an abundant precipitate of lithate of ammonia was found to be present.

9. A portion of the aqueous solution of grape sugar was mixed with a strong solution of lithate of ammonia (artificially prepared), and then a certain quantity of the tartrate test was added, and the mixture boiled. The characteristic precipitate was not produced, but the mixture became of a pale fawn colour. In a weak solution of lithate of ammonia, the characteristic precipitate appeared after boiling the mixture for some minutes. *So that although much sugar is present, the colour of the mixture may be merely changed to brown, and no precipitate whatever may take place.*

10. A solution of grape sugar was treated with a drop of a dilute solution of muriate of ammonia, and boiled with the tartrate of potash and copper test. The mixture became of a brown colour, but no precipitate occurred. Upon the addition of a few drops of solution of potash, the precipitate of suboxide was produced.

A solution of grape sugar treated with Trommer's test, according to the

usual method, behaved in the same way, in the presence of muriate of ammonia, as when treated with the tartrate of copper and potash solution; but in this case a greater quantity of the salt was necessary, for when only traces of the muriate were present, ammoniacal vapours were given off, and the precipitate of suboxide subsided, as before remarked.

From the foregoing experiments the following conclusions, with reference to the practical application of the tartrate of potash and copper test, Barreswil, Fehling, and Trommer's tests, may be drawn.

1. That if the urine contain muriate of ammonia (even in very small quantity), lithate of ammonia, or other ammoniacal salts, the suboxide of copper would not be thrown down if only a small quantity of sugar were present.

2. That unless there be a considerable quantity of the above salts present (in which case the blue colour will remain) the mixture will change to a brownish colour upon boiling, but no precipitate of suboxide of copper will occur. Where only a moderate amount of sugar is present, we have been unable to obtain a precipitate under these circumstances, by the addition of potash to the solution, and prolonged boiling. By observation 8, it appears that a specimen of urine exhibiting this reaction may contain a large quantity of sugar, as ascertained by the yeast test.

3. That in many cases in which the precipitation of the suboxide is prevented by the presence of ammoniacal salts, the addition of potash to the solution, and subsequent boiling, will cause the production of a precipitate, with the evolution of ammoniacal fumes. Hence care should always be taken that there is a considerable excess of free alkali present.

4. When only small quantities of sugar are present, and the precipitate of suboxide of copper is not decided, the fermentation test should be resorted to.

Upon treating different specimens of diabetic urine with the tartrate or with Trommer's test, it has been often noticed that in one case the precipitate is produced as soon as the mixture reaches the boiling point, or even before; while in other instances it is necessary to keep it in active ebullition for some minutes before any precipitate is produced. This circumstance receives explanation from the facts above detailed, with reference to the presence of ammoniacal salts; and other anomalous results which must have occurred to many in the habit of employing this test, become explained.

In those cases, however, in which the suboxide is reduced after boiling for not longer than a minute (or after being allowed to stand for some hours without the application of heat), and falls as a reddish or reddish-yellow precipitate, we have, we believe, as positive evidence of the presence of sugar as can be obtained. As a test for diabetic or grape sugar, if proper cautions be observed, much greater reliance can be placed upon Trommer's or the tartrate of copper and potash tests, than upon simply boiling with liquor potassæ; and in many cases it has advantages over the yeast test, which, however, need not be discussed here. Specimens of urine in which sugar is suspected to be present, and no decided precipitate of suboxide (which must be distinguished from phosphates*) occurs,

* The precipitate of suboxide of copper may be distinguished from the precipitate of phosphates, by its solubility in muriate of ammonia.

should always be carefully fermented with yeast before any conclusion is arrived at.

The facts now pointed out show what extreme care is necessary before the absence of sugar from urine can be determined with any certainty. Equal care is required before the presence of traces of sugar can be affirmed from the application of the tests in present use. We shall, however, if possible, return to the question in our July number, and endeavour, with the aid of additional facts, to form as accurate a judgment on this point as the present state of science will permit.

Lionel Beale.

Review IX.

A Practical Treatise on Diseases of the Skin. By J. Moore Neligan, M.D., M.R.I.A.—*Dublin*, 1852. Small 8vo, pp. 439.

Few Dublin physicians are more favourably known to the profession than Dr. Neligan. The author of an excellent work on the Materia Medica, he has also been intimately associated with Dr. Graves in the preparation of the second edition of his valuable lectures on Clinical Medicine. Nor is the present the first occasion on which Dr. Neligan brings forward observations on diseases of the skin; already, his little treatise on the treatment of Eruptions of the Scalp, has earned for him, in this department, a well-deserved reputation. The evident aim of the author in the work now before us—an aim in which he has undoubtedly succeeded—has been to furnish the student and practitioner of medicine with a thoroughly practical guide to the knowledge of those important diseases of which it treats. Indeed, the greatest value of Dr. Neligan's treatise consists in the plain and thoroughly practical exposition he has given of a class of maladies, the opportunities for the study of which are unfortunately too limited in this country. The magnificent and complete hospitals for the relief of those affected with diseases of the skin, which several of the cities of the Continent, and particularly Paris, afford, contrast most unfavourably for us with the small advantages this country yields. And chiefly on that account the study of these diseases, and the knowledge of their pathology and treatment, have obtained a much more general diffusion among the profession abroad than at home. We are disposed to think that the causes which have led to this are not now likely long to exist; for without raising the question as to the propriety or non-propriety of the establishment of distinct hospitals for particular classes of disease, we think we may safely say that the study of skin-diseases is becoming every day more and more common; and we further think, that in the many excellent works which we now possess in our language on this subject, we have of the truth of this remark a ready confirmation.

Dr. Neligan's volume commences with a chapter on the classification of skin-diseases, a subject of confessedly great importance. His arrangement includes ten groups. 1. Exanthemata; 2. Vesiculæ; 3. Pustulæ; 4. Papulæ; 5. Squamæ; 6. Hypertrophiæ; 7. Hæmorrhagiæ; 8. Maculæ; 9. Cancroides; 10. Dermatophytæ; together with two supplementary groups, Syphilides, and diseases of the cuticular appendages, the Hair and Nails. These various divisions are considered in detail: and in now

endeavouring to present our readers with a very short summary of what Dr. Neligan has written, we shall, for the sake of convenience, follow him in his arrangement.

1. Exanthemata.—From this group Dr. Neligan, contrary to usual practice, excludes rubeola and scarlatina, retaining erysipelas, and classing it along with erythema, urticaria, and roseola, as the components of the division. The reason for thus including erysipelas is not apparent, seeing that its febrile nature, which is allowed by all, and its contagious property, which is admitted by Dr. Neligan, naturally ally it with the two excluded diseases.

Our experience of the erythema nodosum differs in several particulars from that of Dr. Neligan. Unlike his observation, we have repeatedly found this form of erythema in males, chiefly boys. Lately we had an opportunity of seeing a case in the Royal Infirmary of Edinburgh, under the care of Dr. Keiller, in which the disease presented itself in a man of upwards of forty years of age. Dr. Neligan has rarely seen the nodose erythema dependent upon disorder of the menstrual functions; in this particular our own observation leads us to differ from him. No allusion is made to the connexion of this affection with the rheumatic diathesis, yet if carefully looked for, we are satisfied this connexion will in the majority of cases be detected. A remedy which in our hands has proved most useful in this disease, and even in cases where quinine and other medicines had failed, is colchicum, whose anti-rheumatic virtues are well known and appreciated.

In the treatment of erysipelas the author remarks that his own experience is decidedly in favour of the tonic and stimulant plan, though, very properly, the opinion is modified to a certain extent by the statement that the experience referred to was chiefly acquired in a large and crowded city. Notice is also made of the mode of treatment lately proposed by Mr. Hamilton Bell, and his brother, Dr. Charles Bell, of Edinburgh—namely, the exhibition of the tincture of the muriate of iron in repeated doses. Since the publication of the paper of the Messrs. Bell, various corroborative testimonies have been offered, particularly by Dr. Begbie;* and from what we have ourselves seen of its efficacy in the worst cases of the disease, we entertain no doubt of its high value as a remedy, notwithstanding what has been said to the contrary by Professor Bennett and others, who, let it be observed, have never given it a fair trial.

2. Vesiculæ, including eczema, herpes, pemphigus, rupia, and scabies.—Dismissing Willan's group of bullæ altogether, Dr. Neligan has placed rupia among the vesicular diseases, and so doing has, we think, committed an error. If the bullæ are not to exist, which we cannot approve, then much rather had rupia be at once included among the pustulæ: for, although in many instances of rupia the bullar exceeds the pustular character, still in none can that disease be correctly classed with such truly vesicular ones as eczema or herpes.

The huile de cade, of the employment of which in the treatment of chronic eczema Dr. Neligan speaks, we have lately used in a large number of cases, both of eczema and of impetigo, affecting all parts of the body and scalp, and in almost every case with marked success. In a few cases the

* See Monthly Journal of Medical Science for September, 1852.

application of the oil has increased the uneasiness and pain in the affected part, and its use has in such been suspended. We cannot agree with the author in recommending inunctions with the huile de cade to be made twice daily; on the contrary, we feel certain that, so employed, few cures will result; twice or thrice a week is abundantly sufficient; and M. Devergie, whose experience of its use has been the largest, expressly states that more frequent application than that is very apt to magnify the disease. We are entirely at one with Dr. Neligan in his unqualified commendation of alkaline lotions in the treatment of chronic eczema. A word in regard to the employment of arsenic in the treatment of eczema. Dr. Neligan does not speak very sanguinely on this point. Our own experience of its use leads us to do so. Lately, at the suggestion of Dr. Begbie, we have seen most enviable results follow its exhibition to nursing mothers, whose infants had become affected with eczema, in some cases of the face and scalp, and in others of the whole body. In these cases the eruption in the child yielded exactly at the time the physiological effects of the remedy were discernible in the mother. In such cases we strongly recommend the above mode of treatment to our readers, feeling certain that it will not disappoint them.

3. Pustulæ, including acne, impetigo, erythema.
4. Papulæ, including lichen, prurigo.

On these two groups, the various members of which are well described, we have no remarks to make. The observations made by Dr. Neligan on the different methods of cure proposed for these affections are deserving of special attention.

5. Squamæ, including psoriasis, pityriasis.

We approve of Dr. Neligan's regarding lepra as a mere form of the scaly eruption of which psoriasis is the type. We object, however, to the removal of ichthyosis from this class, in which Willan placed it, and to which undoubtedly it belongs. The forms of psoriasis Dr. Neligan recognises. are three—guttata, aggregata, and lepræ-formis. We are glad to find Dr. Neligan speaking strongly in favour of arsenic in the treatment of psoriasis; it is certainly the most powerful remedy we possess.

Psoriasis is frequently rendered a complex disorder, its recognition difficult, and its treatment unsatisfactory, by being associated with other forms of skin disease—for example, with eczema and herpes; indeed, almost all of the simple eruptions, are liable to be thus complicated—a circumstance which ought to be borne in mind, both as regards diagnosis and treatment, yet one which, in systematic works on diseases of the skin, is not sufficiently dwelt upon, and to which Dr. Neligan too seldom alludes. There is no great difficulty in recognising a case of eczema or a case of psoriasis, and no great mystery in directing the treatment of either; but to establish easily and quickly, and that from a close observation of its characters, a case of eczema impetiginodes, or one of psoriasis herpetiformis, is a more difficult task, yet not one less likely to occur, for all varieties of the complex eruptions are sufficiently common.

The following are the remaining five groups of Dr. Neligan's arrangement:

6. Hypertrophiæ, including ichthyosis molluscum, stearrhoæ, elephantiasis verruca, clavus, nævus.

7. Hæmorrhagiæ, purpura.
8. Maculæ, including vitiligo, and ephelis.
9. Cancroides, lupus, and keloës.
10. Dermatophytæ, porrigo, sycosis.

We must not omit to mention that the concluding chapter of the volume is devoted to some interesting remarks on the therapeutics of diseases of the skin. Dr. Neligan's work we can recommend both to student and practitioner, as a useful guide to the knowledge and treatment of cutaneous diseases.

J. Warburton Begbie.

Review X.

1. *Die Eigenthümlichkeiten des Kindlichen Organismus im gesunden und kranken Zustande. Eine Propädentik der speciellen Kinderheilkunde.* Von Dr. Daniel Gottlob Moritz Schreber, Vorsteher der Orthopädischen Heilanstalt zu Leipzig.

The Peculiarities of the Organism of the Child, in the Conditions of Health and of Disease. A Propædentic to Special Pædiatrics. By Dr. D. G. Moritz Schreber, Director of the Orthopædic Institution at Leipsic. —*Leipsic*, 1852. 8vo, pp. 118.

2. *Ueber den Durchfall der Kinder.* Von Dr. C. F. Eichstedt, Assistenz-arzte bei der geburtshülflichen Klinik zu Greifswald, &c. &c.

On the Diarrhœa of Children. By Dr. C. F. Eichstedt, Assistant-physician at the Maternity Charity of Greifswald.—*Greifswald*, 1852. 8vo, pp. 131.

3. *Die Krankheiten der Neugebornen und Säuglinge vom klinischen und pathologisch-anatomischen Standpunkte bearbeitet.* Von Alois Bednar, Dr. der Medecin, Vorm. prov. Primar-artze des K.K. Findelhauses in Wien, &c. &c. Dritter Theil. Krankheiten der Respirations-organe, &c. &c.

The Diseases of New-born Children, and of Children at the Breast, Viewed in Relation to Clinical Medicine and Pathologic Anatomy. By Alois Bednar, M.D., &c. Part III. Diseases of the Organs of Respiration, &c.—*Vienna*, 1852. 8vo, pp. 208.

4. *Journal für Kinderkrankheiten.* Herausgegeben von Drn. Behrend und Hildebrand.—Band XVIII., Heft 3 u. 4.

Journal for the Diseases of Children. Edited by Drs. Behrend and Hildebrand.—Vol. XVIII., Numb. 3 and 4.—*Erlangen*, 1852. 8vo, pp. 156.

We had intended, if sufficient space had been at our command, to have made some general remarks upon those circumstances which appear to us to have operated in causing the more eminent writers on the diseases &c. of children, to limit their systematic treatises to the consideration of the individual maladies of infancy and childhood, and to withhold from us any distinct or substantive work upon the general physiologic and pathologic peculiarities which distinguish the youthful from the adult frame.

Re infectâ, however, we may just observe that although, in connexion with the latter subject, the reader may refer with much profit to the opening chapters of the new edition of Bouchut, and to those appended to the well-known and elaborate work of MM. Rilliet and Barthez, as also to the introductory remarks in a few other systematic treatises; yet it cannot be denied that the want of such a separate work as we have alluded to from one well known in our present *spécialité*, has been a chief cause why the subject has fallen into the hands of both *extra* and *intra*-professional charlatanism, and we have become overridden with so many 'Mother's Guides,' 'Lectures on the Management of Infancy,' &c. &c., which deal either with the very vaguest and most puerile generalities, or with such topics and recommendations as prove their authors to have had alone in view the recommendation of Iago, "Put money in thy purse." Be this as it may, however, Dr. Schreber, participating in some of our opinions, and apparently having no faith in the value of any semi-popular and professional 'Guides to the Nurse and Nursery, (advertising media, we know from experience, his own country, like this one, patronizes,) has thought it advisable to offer a scientific sketch that might be of service to the student as an introduction to the study of special pædiatrics."

"The scope of this work extends only to that point where the office of the latter study commences, and with which of course it stands in immediate connexion It is intended to assist in establishing that foreknowledge which the practitioner must possess if he would apply himself with a hope of success to the study and practice of medicine as connected with children. The system of the child in its whole essence offers so much to the practitioner, and so many peculiarities and specialities as relates to practice, that the attempt to establish a 'propædeutic to special pædiatrics' may be well justified." (Pref. iv.)

Dr. Schreber divides his treatise into five sections, the titles of one or two of which do not certainly very clearly indicate what is contained under them. The first section is entitled "Physiologico-practical Fundamental Propositions;" the second, "Dietetics of Childhood in their general Indications;" the third, "General Propositions and Rules relative to the Management of the Diseases of Children;" the fourth, "General Observations on the Eruption and Treatment of such Affections which, although common to the Adult as well as to the Child, offer, in the latter, marked peculiarities;" the fifth and last treats of Dentition, and some allied subjects. Upon the first section, we shall offer no comment—not that it admits of none, but in regard to our limits. The second refers to the general hygiene of the child, the author introducing the subject by some remarks relative to that of the mother, as the only medium by which the child can be influenced during its intra-uterine existence.

"By such prophylactic precaution alone is it in the power of the physician to affect the fœtus. All attempts and recommendations for operating curatively upon the diseases of the fœtus have hitherto failed, and evidently upon the double ground, first, because the diagnosis of its affections is not yet established upon a sound basis; and secondly, because experience hitherto has shown that we possess no sure and previously-to-be-reckoned-upon method by which we can exert our therapeutics or employ our drugs. Nevertheless, many persons have been misled by an appearance of the possibility of such, and have erroneously imagined that particular remedial measures (as the antiphlogistic, especially loss of blood, alteratives, &c.,) must, through the maternal organism, operate upon the fœtal one just in the same way as they do upon the first." (p. 27.)

M. Devilliers seems to be of this opinion, since in the abstract given of his communication to the Academy in Paris upon the syphilis of pregnant women, in the 'Journal für Kinderkrankeiten,' he is reported to have said that

"In every month of pregnancy the antisyphilitic treatment of the mother and child seems to be the better borne the more complicated and serious the syphilitic symptoms. The primary, as also the secondary ones, which show themselves during the latter weeks of gestation, require not only a local treatment (especially when having their seat on the genitals of the mother), but a general one, from which it at all events results, that the child at birth, if not healed, comes into the world with milder symptoms, and more readily supports the continued mercurialization, if under such circumstances it should appear necessary." (p. 312.)

Dr. Schreber's remarks upon diet, clothing, sleep, education, &c., are all very good so far as they go, but offer nothing novel or sufficiently important to detain us. We find place, however, for the following note concluding the section:

"The distinctive characters of a prematurely born child (viability being considered possible from the thirtieth week of pregnancy) are the following: whilst the body is generally smaller and more delicate, the head is unproportionably large, the skin of a deep red colour and still covered with soft hair, the power of generating animal warmth low, the nails of the fingers and toes soft and imperfectly developed, the movements weak, and the power of respiring and sucking below par. The latter at least is not continuous, without cessation or pause, the cry is weak or whimpering, and the child is almost always asleep. According to the numerous measurements of Outrepouts ('Gemeins. deutsche Zeitschr. f. Geburtsk., B. iv. H. 1, S. 558) the position of the umbilicus affords a sure criterion. In children born at the full time it is placed exactly midway between the vertex and sole of the foot; its position away and lower down from this middle point is in a ratio with the prematureness of the child." (p. 44.)

When we direct our attention to this point in a cursory examination, the small size of the little finger, and of its nail especially, and the tendency of the child to curl up and cross its legs in a fœtal-like manner, are the signs to which we generally refer.

The third section of Dr. Schreber's work is, in our opinion, the best, and will well repay perusal, particularly the subdivisions on "Methodics" and "Semeiotics" While perusing our author's "Therapeutics," we could not help recalling to mind the able little work of Dr. Beck, noticed by us in a former number, and which made us wish that Dr. Schreber had not kept himself within such a limited scope as relates to this subject. From the "Methodics" we extract the following, well knowing the value of the caution urged in it:

"In consequence of the greater mutability of the child's system, by which one disorder easily passes into or becomes complicated with another, and from the, in general, more deceptive symptoms of its disorders, the *prognosis* should be given with the greatest circumspection; and whenever it appears necessary, it is advisable to inform the relatives of the patient (at least the more prudent of them) at the outset, of the possibility of the supervention of the usual modifications and complications of the disease. Inattention to this rule will soon destroy the reputation of any desirous of being thought conversant with the diseases of children. Relative to it one of the most difficult points of our 'methodics' is seen, for the physician of a sick child has particularly to be aware that he is the source of alone the most soothing influence in everything connected with his little patient." (p. 48.)

Those conversant with the maladies we are discussing, cannot fail to have been struck with the very rapid and extreme emaciation which frequently accompanies many of them, particularly remittent fever complicated with diarrhœa, occurring in the summer months. What with the loss of appetite preventing nourishment being taken, the sickness, the discharge from the bowels, and the systemic erethism, the marasmus becomes the point of most anxiety to the parents, and consequently the one most urged by them on the notice of the practitioner. On the part of the latter, it is requisite he possess the full confidence of the relatives ere they will believe the justice of his assertion, that such symptom is not of chief importance in his mind. The recollection of the bilious remittent (with its tendency to jaundice) of the past summer, forcibly reminds us of the truth of the above. In his "Semeiotics," Dr. Schreber remarks:

"In all diseases associated with diminished nutrition the child emaciates far more quickly than the adult, but, on the other hand, the former regains its condition much sooner than the latter. Such emaciation—if the disease is not very dangerous—is of no great import so long as the countenance does not exhibit marked change; but if it does, if it assumes an unnatural, more aged, thin, wrinkled appearance, we may always reckon upon a deep and danger-threatening diminution of reproduction and of the whole vital power." (p. 55.)

It is stated (p. 62), that great dilatation of the pupils, with bluish rings beneath the eyelids, is a common symptom of the presence of intestinal worms. That largely dilated pupils with bluish irides are frequent in the xanthous variety of scrofulous children, and that in them intestinal parasites are very frequent, are two facts of which we are well aware. But that the former statement, applied generally and purely in reference to verminous disorders, is true, we are loath to admit. That general bloodletting has a different relative effect, *cæteris paribus*, upon the young child, to what it has upon the adult, is universally admitted. When carried to any extent, the nervous system is more powerfully affected; and if syncope should happen to supervene, convulsions and coma may terminate the matter. A repetition of general bloodletting is also in general detrimental, although the first venæsection may have produced no ill effect. Under any circumstances, too, the recovery from the effects of loss of blood, though the latter may have been of avail in arresting the malady, is slower and more difficult than in the adult. These, and other circumstances we need not now allude to, with the exception of the fact that local depletion has relatively more influence upon the child than upon older persons, must incline one to agree with the doctrine taught by our author (p. 78), that, except under peculiar and very urgent circumstances, local bleeding is to be had recourse to in young children in preference to general; the more particularly as the former, under proper management, is in *most* cases a weapon of such force that no stronger should be employed. Whilst we have no hesitation in maintaining this doctrine, we would express our belief that such *local* depletion may be often substituted with great advantage for the antimonial and mercurial &c. depressants by which alone, in many cases, particular affections are therapeutically met. The disease is more quickly, and we think effectually arrested, a relapse less probable, and the patient and the relatives less troubled in the long run, than by the constant administration of the above remedies, every two or three hours in

the day. Amongst the lower classes of society, too, very little dependence is often to be placed upon their giving properly what has been ordered; though at the same time we admit that serious results often arise from their mismanagement of leeches. Upon this point we have before touched when reviewing the work of Dr. Churchill.* If general bloodletting is of limited application in the young child, blistering by cantharides plaster is not less so. This statement we intend more particularly to apply to the treatment of thoracic inflammation generally, and with especial force to it when occurring in the course of the exanthemata. Blisters may be applied behind the ears, even in very young children, with—as a rule—impunity, but on the sternum or between the scapulæ they prove generally as bad as the disease (and often much worse), for which they have been employed. Too often have we seen the child sink from the want of sleep, local and general irritation, thus given rise to; sloughing of the sore is by no means uncommon, and altogether the practice should be, as far as possible, abrogated. Even in good hands, employed by those who are aware that they produce their effects here in a shorter time than in the adult—that the local inflammation is greater, the constitutional erethism they give rise to of a higher grade, and their primary action far more likely to be followed by the secondary consequences of ulceration and gangrene, and thus only allowed to remain on the skin but a very short time—we have known them produce the greatest anxiety in the mind of the practitioner when he has witnessed the result of the application of the emplastrum cantharidis for one hour and a half. We are of full agreement, therefore, with the general tenour of Dr. Schreber's advice (p. 80), but not so with the particular dislike he appears to have to the use of sinapisms. We prefer mustard as a counter-irritant in the young child, to any softened or modified preparation of the cantharis vesicatoria. Our author concludes his "Therapeutics" by some remarks upon the value of the "cold and warm water cure," as applied to children. The fourth section of his work is very well, so far as it goes; but the observations are of so general a character, as to offer no reason for our detaining our readers by any comments on them. The subject of syphilis receives more notice. The fifth and last section is mainly occupied with the question, "Whether and how far is the process of dentition to be considered of pathogenetic account?" In this country at least it has been long since answered in the affirmative; and with Dr. Schreber we entirely differ from those who deny that dentition may become of "pathogenetic account." Such persons—and we may take Dr. Brefeld† as their fitting representative—assert, that, "like all other universal developmental and formative epochs, dentition is a normal physiologic process, necessarily involved in the idea of organic life." This proposition may be freely admitted. But not so a conclusion which involves the admission of another (the suppressed) proposition—viz., that no normal physiologic process &c. can become of pathologic import, and therefore (the conclusion) that dentition cannot; or, as Dr. Schreber puts it—

"Is not the ordinary regeneration or moulting-process (including the decadency of the hair, skin, shell, &c.) of animals quite in a natural condition, well known to

* Vol. vi. p. 145, *et seq.*

† Dentitio difficilis—das gefährlichste aller medicinischen Vorurtheile. Hamm. 1840.

be often accompanied by more or less pathologic phenomena? In the human race, is not the appearance of puberty, are not pregnancy, parturition, the puerperal state, decrepitude, clearly pathologic processes, and yet admitted by all practitioners as capable of becoming of pathogenetic moment?" (p. 113.)

Upon what ground, then, is it to be maintained that dentition cannot? For further remarks we must refer to Dr. Schreber. In parting from him we would say, that although his work is of too slight and sketchy a character to fill up the void we have alluded to and he has aimed at closing, yet it is good in its intentions, able so far as it extends, and a fitting, though a scanty, introduction to the wide field of special pædiatrics.

If we stated that our present branch of medicine had now attained a development equal to any other, we should intend, of course, the assertion to be of general application only, well knowing that in respect to certain points of detail, it is yet behind in that precision which it is necessary to look for. Were a student, after having spent a few mornings in the month of August at a dispensary for children, and witnessed the numerous cases of a disease which would then prevail—viz. diarrhœa—to return home and seek to renew his acquaintance with it as afforded by books, we are sure he would very soon be sorely puzzled. In one work he would be told of the importance of diarrhœa as a special substantive disease, and all sorts of modifications or varieties of it explained to him. In another he would not discover the word at all employed as the sign of a special disorder; he would be told that there was no such disease *per se*, but that it was simply the name of a single symptom of several allied maladies, and which latter he must study, and which he must treat. He would find it by one asserted that instead of diarrhœa, he must think of general or local inflammation of the intestinal mucous membrane, or of ulceration of its follicles; or be directed to accept the doctrine that it is one of the aggregate of symptoms attendant upon some "phlegmasie ou ramollissement" of the linings of the digestive tube. On the other hand, it would be hinted to him by another, that whilst in many cases the above statement would hold good, it would not in all; and that it is not always easy to say during life which would be correct. It would be shown that the same lesion is common to many forms of diarrhœa; or that the lesion generally met with in a particular variety may be wanting after death, though during life we might have fairly inferred its existence. Nor would such views embrace all that he would meet with; he might still be assured that the best view of the matter is to accept only two chief forms of the disorder—viz., a functional or catarrhal diarrhœa, or a diarrhœa of irritation, and an enteritic or inflammatory one, a diarrhœa of organic lesion. Whilst another would produce proof that from the examination of no less than 336 cases, he could only come to the conclusion that "hyperhæmia of the brain and its membranes, anæmia of the lungs and of the liver, and the viscid exudations of the serous membranes, constitute the anatomical results found as proper to diarrhœa;"* that there is no such thing to be demonstrated in infants, either microscopically or anatomically, as catarrh of the intestinal mucous membrane, and that its chronic forms, or "muco-enteritis chronica,"

* Bednar, Part I, reviewed in vol. vii. p. 112.

must be described as a symptom of "tabes;" that he has never seen "dysentery" in children at the breast; and that in all probability the chief cause of their diarrhœa is to be sought in "a kind of fermentation," "a primary abnormal process of decomposition of the contents of the stomach and intestines."* Lastly, our new friend, Dr. Eichstedt, would impress upon our noviciate in pædiatrics, that in diarrhœa not of a chronic character, "dissections have afforded very various results; that no change was constant, except viscidity of the blood in obstinate cases" (p. 31); and that "the massing together by Bednar of the diarrhœas of children as one disease, only variable in degree, can only have, so far as treatment is concerned, the most prejudicial consequences" (p. 66). Now, considering the high authorities who have treated this subject, and, moreover, knowing the difficulties by which the exact elucidation of this apparently simple malady, diarrhœa, is surrounded, we would wish it to be understood that we speak with all deference of intention, if not of terms. It is evident that much of the discrepancy of opinion which exists has its origin, like many other things, in a defective analysis, and consequently too limited a generalization, on the one hand, and in a too sweeping synthesis and too universal a generalization, on the other. If Rosèn von Rosenstein, with his fourteen kinds of diarrhœa, one from overloading the stomach, another from acridity of bile, one from the retrocession of cutaneous eruptions, &c. &c., may represent an instance of the former; Rilliet and Barthez, in describing the results of their observations on affections of the digestive tube, under the terms of gastro-intestinal inflammation and typhoid fever, afford an example of the latter. In the present state of our knowledge, and as sufficient for all practical purposes, we may ask the following questions:—1st. Is not the disorder usually understood as diarrhœa frequently presented to us in such a form and under such circumstances as to justify us fully in viewing and treating it as a substantive affection, and this without directing our attention to any undeniable specific or constant anatomic lesion of the digestive mucous lining or its appendages, as its cause? 2ndly. Are there not other forms, often wrongly considered as representing a purely functional yet substantive disease, which should rather be regarded as a severe and important symptom of a malady having as one of its lesional characteristics variable grades of inflammation, and its different results, of the intestinal mucous membrane and its appendages? We would answer both in the affirmative. In respect to the first, it may be argued against us that, in viewing certain forms of diarrhœa as a purely functional as well as substantive malady, we are disregarding the apophthegm of Legendre,† and which has so fully received the sanction of the precise pathology of the day. We would reply, it must be so then, seeing, 1st, That the nature of the causes of these forms of the affection, the course and symptoms of the latter, their temporary character, their amenability to particular forms of treatment, forbid us to believe we have to combat with inflammatory action, with its results, or with any definable anatomic lesion. 2ndly. That post-mortem investigations, where a patient has had diarrhœa, but has died from another malady, absolutely show that no such intestinal organic lesions exist, and that even in some severe forms of the disorder,

* Bednar, op. cit.

† "C'est par l'altération des organes que l'on doit caractériser et spécifier les maladies."

where life seems to have succumbed to the violence of the diarrhœa, the intestinal changes bear no sort of proportion to the symptoms previously existing. And, 3rdly, As we believe that a refined analysis will inevitably lead us to the doctrine that all affections, not what may be termed traumatic in their character, are primarily functional, that is to say, originally dependent on some modification, alteration, or disturbance of *power*, we find ourselves under no such absolute necessity of discovering, as the essential nature of every malady, some material, visible, structural change; the more particularly as we too frequently see such change considered as *the* disease, or at least giving a name to it, when after all it is only the effect or result of a disease, and which latter is something else altogether different in its nature. So far, then, as the above propositions hold good, we feel justified in maintaining an affirmative to the first question; so far as they do not, and for the following reasons, we yield it to the second:—1st. Because the causes, extrinsic and intrinsic, of the forms of the disorder there referred to, the intensity, complications, progress, and duration of the latter, the form of treatment to which they are most amenable, or by which they are best palliated, lead us to believe that with the functional disturbance has co-existed from the first a distinct organic change,—that this disturbance becomes the more severe the greater this change, and that treatment is generally the more successful the more it is directed in reference to such change. 2ndly. Because post-mortem investigations evince the existence of morbid lesions, which we may fairly associate with the previous diarrhœa. Now that this affection must be regarded in this twofold light, we have quite sufficient personal experience for believing; but that it is an easy matter always to affirm of every diarrhœa under which category it is to be reckoned, we are far from maintaining. But this is not uncommon in other things: we know the general law, and are able to express it with a fair universality of application, but yet feel puzzled in reducing certain apparent cases within it, although we feel satisfied that it might be done, notwithstanding. As applied to disease, we infer from certain symptoms, &c., according to a general law, the want of or the existence of certain organic changes after death. In a particular case our inference is proved wrong. Where is the error? Have we interpreted the symptoms wrongly, and therefore the inference is not right; or did we seize them in all their bearings, give them their usual meaning, and yet they failed to speak the language they generally utter? Be the error where it may, we admit, as we before remarked, it is liable to be made when reasoning upon certain cases of diarrhœa, and therefore may be taken as another reason for the discrepancy in opinion which prevails respecting the latter. Thus Dr. West remarks, in proposing to distinguish between simple and inflammatory forms of it:

"I yet was forced to acknowledge that the distinction was one rather of degree than of kind, or perhaps it would be more correct to say, that our observation has not hitherto been minute enough to enable us to draw the line of demarcation strictly between the two affections. (Lect. p. 432.)

And Rilliet and Barthez, that—

"Not only are the lesions answering to the same symptoms sufficiently different, but, also, there is not always to be found a proportionate intensity between the one and the other; for whilst on the one hand we may have entero-colitis severe or

slight in its symptoms during life, and its lesions equivalently so after death, we find other cases very intense, symptomatically, and but slight, anatomically considered, and likewise the converse.*"

Nor should the remarks of Bouchut† or of Bednar‡ and others, be forgotten. Had our limits permitted, we would have dilated somewhat upon the symptomatic characters of the two great divisions under which we have viewed this affection, and the anatomic alterations characterizing one of them. As it is, we must now allow Dr. Eichstedt to come under observation, premising that upon the morbid anatomy of diarrhœa he offers nothing that is new; on the contrary, it seems to have lain beyond his province. In his preface he informs us—

"Having had no opportunity for making numerous dissections, and being anxious to substantiate the more prominent effects of particular agents upon special portions of the intestinal canal, I instituted experiments upon rabbits." (p. ii.)

That our author participates in some of our own views is apparent from his commencement:

"By diarrhœa is understood the frequent evacuation of watery excrementitial matter from the anus. In recent times, instead of being regarded as a special disease, it has been looked upon as the symptom of various morbid conditions, and treated accordingly. Though I may not be disposed in the least to withstand this opinion, yet it appears to me, practical advantage will be found in a closer consideration and comparison of those circumstances in which diarrhœa appears as the chief symptom of the disease, the more particularly as in infancy it is often the only appreciable morbid phenomenon. It would be out of place here to enter more fully upon the subject, in respect to such cases where an important malady like pneumonia, or typhus, or tuberculosis, is present, and where the diarrhœa appears simply as a subordinate circumstance. In the adult the different forms of the affection are by no means of the same import, nor do they offer such peculiarities as in infancy, to which period, therefore, I shall confine my attention. Every practitioner will certainly agree with me in admitting the high importance of this abnormal condition in children, partly from the frequency of its occurrence, partly from the great danger arising to the little patient, particularly when it is prolonged, and the treatment at the beginning not exactly what it should have been.

"Before passing to the special investigation of it, it may be proper to allude to the anatomic relations of the digestive canal, so far as they are connected with our present subject." (p. 1)

The observations of Dr. Eichstedt upon the latter we shall pass by, simply remarking that the structural peculiarities of the intestinal tube, the physiological incidents of digestion, the properties of the fæcal evacuations, &c., are all ably discussed. A general description of the symptoms and course of the acuter form of diarrhœa next follows; that of the chronic is afterwards given. Speaking of the amelioration of simple functional diarrhœa, it is observed—

"Unfortunately this favourable issue does not always follow; on the contrary, danger may arise in a twofold way, either by the supervention of brain affection, very easily induced as exhaustion comes on, or by the transition into inflammation of the intestines, most usually of the larger."

"If the disorder is not soon relieved the signs of exhaustion are quickly evinced, as seen in the sunken condition of the eyes, encircled by bluish rings, and in the

* Maladies des Enfans, tom. i. p. 510. † Malad. des Nouveaux-Nés, p. 524.
‡ Krankh. d. Neugeb, tom. i. s. 6, 46.

bluish colour of the lips. In children in the state of exhaustion, blueness of the skin soon comes on, as also coldness of it, particularly at the extremities. The fontanels sink and become elevated only when the child cries. Convulsions are now superadded, as likewise tendency to sopor, and the child dies, exhibiting the phenomena of acute hydrocephalus. Sometimes convulsions with their untoward termination supervene earlier, even at a time when the child is quite lively and the disorder considered as of no great importance." (p. 28.)

For the symptoms indicating the transition into inflammation, we must refer to the author's pages. Following these we have a very short account of the post-mortem appearances, but a long one of Dr. Eichstedt's experiments upon his rabbits. The *résumé* of these we shall of course give, as our author appears to pride himself somewhat on them. Fourteen rabbits were employed, and were dosed with cambogo, colocynth, hellebore, aloes, croton oil, rhubarb, and jalap.

"The result of these investigations is the deduction that all purgatives do not operate equally strong upon all portions of the intestines. Camboge and aloes had evidently exerted their chief influence upon the follicles of the larger bowel, croton oil influenced particularly the duodenum and jejunum, whilst colocynth, as it appeared to me, operated upon the nervous system without affecting in a high degree the mucous membrane of the canal. Rhubarb seemed in particular to excite the small intestine to increased action, as did also jalap, although in both cases a stronger flow of blood to the brain was likewise observed. Calomel appears to be converted into the sublimate in the stomach of the rabbit, and thus to acquire a corrosive power. That the loss of vitality to the mucous membrane arose from such local causticity, and that the ulceration was owing to the detachment of the membrane, there can be no doubt." (p. 39.)

In the tenth case the rabbit was dosed with ʒiss. of rhubarb; no pultaceous or frothy evacuation had taken place by the next day; it was then soporified by æther, and its abdominal cavity opened. The peristaltic movement soon became very strong, and intussusception followed. The process of its formation was ocularly observed by Dr. Eichstedt, who regards it as one of the most interesting observations he had the opportunity of making.

"A portion of the small intestine contracted itself quite closely, and continued some time in this state; the upper part of the bowel then commenced strong peristaltic action, and soon afterwards the part beneath and contiguous to the contracted portion assumed an antiperistaltic movement. As the peristaltic was stronger than the antiperistaltic motion, the contracted portion became pushed into the part moving antiperistaltically. I witnessed five such intussusceptions, in all the process being the same; after some time they became loosened. In one the contracted portion was pushed a full inch deep into the under bowel." (p. 38.)

It is well known to those who have examined the bodies of many children, that invagination or intussusception is not unfrequently met with in them; but, from the diseases of which such children have died being very variable, often no abdominal disturbance to speak of having been present during life, and from the intestines betraying no signs of inflammatory action, although as many as a dozen invaginations are said to have been found, it has been fairly presumed that they have arisen in the agony or moment of death. But this lesion is known to occur, and we have met with it, under two other circumstances—in the course of inflammatory diarrhœa, and as a primary disease—which, in the case of the latter variety

occurring to us, was followed by peritonitis. Louis is reported* to have seen 300 cases of invagination among infants at the Salpêtrière dying during the travail of dentition or "*d'accidents vermineux.*" But as in these cases no appreciable symptoms of such a lesion were present during life, they evidently belong to the form of the affection first alluded to. Instances of the other varieties are by no means common, as may be judged of from the statement of Dr. West, that it has never happened to him to meet with a fatal case, and but only once to have observed an instance in which the symptoms of intus-susception having existed in a marked degree, at length spontaneously ceased, and were followed by the restoration of the infant to perfect health. (Lect. p. 424.) Still scattered amongst serials will be found the detail of numerous cases, nine being alluded to alone in Mr. Gorham's 'Observations,' published in the 'Guy's Hosp. Reports' in 1838. Indeed to this gentleman is due the credit of a very able analysis of the symptoms &c. of the disease in question; and it was chiefly by attention to the diagnostic sign laid down in his work that we were able to arrive at a correct diagnosis in the two cases we attended. This sign is "the passing of blood per anum in various degrees of purity, never indeed contaminated with fæculent matter, but chiefly with mucus," and to which should be added, constipation, vomiting, and constant straining as if a motion was to pass, nothing however but blood being seen, from a few drops in some cases to truly hæmorrhage in others. Of the two cases coming before our own notice, one was in a male child of eleven months, suffering under entero-colitis. No food was taken for several days before death; there was constipation, much vomiting, and frequent straining, with the passing of a little blood. The other was also a boy, æt. four months. He was brought to us two days before death, and described as having had no fæcal evacuation for five days, but vomiting and straining, the latter frequently accompanied by small bloody and slimy discharges. The abdomen became tympanitic and tender, and the child fell rapidly into a state of collapse. Intus-susception was diagnosed here, as in the other case. In both, the necroscopsies substantiated the diagnosis, as well as that of peritonitis in the latter. In both of these cases, and in another, that of a young lady, in whom it was assumed, from the symptoms only, that invagination had occurred, the plan of injecting large quantities of fluid (in the latter up a long tube) was tried: in neither was it of any avail, but, on the contrary, seemed to be a source of much trouble, if not pain, to the patient. We shall probably recur to this subject at a future period, as there is much to say upon it.

The subject of Ætiology is next and well discussed. The effects of local irritation on the alimentary canal, as from bad maternal milk, faulty diet, too hot and fermenting aliments and drinks, substances producing increased secretion of bile, agents having a direct purgative action of their own, and circumstances operating physically, are severally considered. The influence of dentition, cold, &c., applied to the surface, epidemic causes, and contagion, then follow. In this portion of his book, Dr. Eichstedt introduces, unnecessarily, as we think, a long account of one of his *hobbies*—viz., the structure and functions of the secreting organs of the skin. Of the correctness of his views upon these points we will offer no opinion, but leave

* By Hevin, in his Memoir on Gastrotomy, and referred to by Bouchut. Mal. des Nouveaux-Nés, p. 585.

him to the care of the anatomists and physiologists; but we cannot avoid remarking, that if the following be the chief practical result our friend has arrived at, we must place him by the side of that famous historian, who discovered, after much research, that "the Dutch had taken Holland."

"The whole external skin, viewed particularly as a sweat-secreting organ, stands in close antagonistic relationship to the intestinal canal; so that when suppression of the perspiration ensues, increased secretion from the intestinal mucous membrane very readily supervenes, and on a sudden hindrance to the increased secretion from the bowels the perspiration is augmented. Such alternating circumstances are frequently observed during the course of a malady such as phthisis. In this way 'taking cold' operates, the cutaneous exhalation is depressed, and in lieu of it the intestinal secretion is augmented." (p. 51)

The consideration of the symptoms and treatment of the different forms of diarrhœa established by the author, next occupies attention. For systematizing the latter, Dr Eichstedt has evidently some leaning towards a division, depending upon the character of the stools. A sketch of such a classification is given, and a description of its various forms—viz., diarrhœa fæculenta, biliosa, serosa, mucosa, puriformis, chylosa, sanguinea, and lienterica (p. 60). Now, if we are to have a classification of this kind, we should prefer Trousseau's, with the addition of one form, the D. serosa. However, Dr. Eichstedt himself, scarcely satisfied with this principle of division, and believing that the best of all for practical purposes is that which most prominently brings forward "the *periods* when we have to do with their treatment" (p. 67), finally adopts the following arrangement:

1. Diarrhœa previous to *dentition*. { *a.* Simple diarrhœa. *b.* Diarrhœa tending to inflammation of the mucous membrane.
2. *Diarrhœa of dentition.*
3. *Diarrhœa of weaning.*
4. Diarrhœa caused by epidemic *influences*. { *a.* Epidemic. *b.* Dysenteric. *c.* Choloric.
5. *Chronic diarrhœa.*

Whatever opinion we may hold upon some minor points of our author's views, we can strongly recommend this portion of his treatise, as containing much valuable practical information; as an example, we select the following:

"If the diarrhœa has been caused by improper food it will often be necessary to remove such matters, indigestible by the child, by gentle purgatives. For this purpose, a combination of magnesia with rhubarb answers very well. If a decided tendency to the formation of acid has already shown itself, it will be necessary to neutralize the superfluous acid, or remove it from the stomach, and destroy such tendency. For its neutralization, magnesia usta, or the carbonate, is most frequently used, in union with rhubarb, to carry it off. Bressler regards magnesia as lying too heavy upon the stomach, and as not always the best remedy against acidity. He prefers equal parts of aq. calcis, and some aromatic water, in teaspoonful doses, or the liq. kali carb., very cautiously administered in small quantities. As antacids, the hydrate of alumina, and wood charcoal are to be particularly recommended. The former, first advised by Percival, then approved of by Ficinus, has been strongly advocated by Weese and others. If much acid be present, mere neutralization will not suffice; it will be proper that an emetic be first given, and then magnesia, with rhubarb, administered. The use of emetics

in young children has found many advocates, especially in former times. They were chiefly recommended in the diarrhœa of children by Armstrong and Schæfer, the former employing tartar emetic every six to eight hours, until the evacuations improved. Schæfer agrees with this, and remarks: 'Very many, indeed I might almost say most, of the diseases of new-born and young children require repeated evacuations, and particularly the employment of emetics.' In modern times ipecacuanha has been preferred, as by Trousseau, Versoif, and others. It is often necessary to lessen the increased sensibility of the intestinal mucous lining, caused by the irritant action of the acid; for this saffron best suffices, and which can be fitly conjoined with rhubarb and magnesia. Nux vomica, in small doses, has often been found useful in these cases, but opium should never be resorted to After the evacuation of the acid contents, the tendency to the formation of new acid must be obviated; the diet must therefore be changed; milk must be entirely avoided for a few days. The more useful remedial agents will be found to be the chloride of calcium and the carbonate of ammonia, in small doses; by these the disposition to the generation of acid is often quite removed in a short time. If such be effected, no gastric impurities being now present, then the diarrhœa, if continuing, may be met by astringent remedies, amongst which, calumba, the extract of cascarilla, and lime-water, with milk, have proved the best." (p. 75.)

The treatment thought most highly of by the author, where inflammatory symptoms follow the simpler forms of the malady, consists of leeches to the abdomen, and calomel frequently repeated in small doses. The epidemic varieties of diarrhœa are illustrated by an account chiefly of an outbreak of the disorder which occurred at Greifswald, in the summer of 1842. The few experiments made by Dr. Eichstedt in feeding rabbits and a hen with matter from the evacuations of cholera patients, should be compared with our abstract of M. Renault's, in our last July number, page 252. The former thinks he may draw at least the deduction that an agent was present in the evacuated material, which operated particularly energetic on the nervous system of such animals as he administered it to. The work terminates with the consideration of chronic diarrhœa. It is one of considerable practical value, and contains much valuable information in a comparatively small compass. Upon some litigated points of morbid anatomy, it gives us, it is true, no new information; indeed, this subject is the weaker point of the book; but it contains physiological information of much interest in connexion with the disorder it treats of; and the ætiology, symptomatology, and therapeutics, of the latter are well worked out. We could have wished to have dealt more fully with Dr. Eichstedt.

In our review of Dr. Weber's work* we drew attention to some of the more interesting points in reference to *cephalhæmatoma*. Opinions being various still as regards one or two circumstances connected with the subject, we would further direct our readers to an able paper† by Professor Levy, of Copenhagen, in the 'Journal für Kinderkrankheiten.' It is based upon the personal observation of fourteen cases, aided by reference to the experience of other observers. We shall give a short analytic *résumé* of the more important conclusions arrived at by the author. These are, 1st, that too much stress is not to be laid on the assertion of different writers, that the affection is chiefly met with in primiparæ, since out of fourteen cases, four were not such; 2ndly, that there is even less reason for believing its frequency of occurrence in boys to much exceed that in girls, as the ratio was only 8—6; 3rdly, that the views of Valleix, as to its

* Beiträge zur Pathologischen Anatomie der Neugebornen Rev., vol. x p. 6.
† Beobachtungen über des Kephalamatom, &c. (p. 161 of Journal.)

usual origin or ordinary mode of formation are probably incorrect, the disease being rather due to a strong resisting force applied to a particular portion of the head, by some part of the bones of the pelvis, varying according to the point of greatest frictional pressure during labour. The assumed cause of Valleix—viz., circular pressure by the neck of the uterus, only produces subpericranial ecchymosis, between which and cephalhæmatoma, the difference, although in one respect scarcely more than in degree, is symptomatically very great. The former is only known to exist by post-mortem investigation, and is of importance practically to the medical jurist only. 4thly. In respect to diagnosis, there are two points demanding attentive consideration. The first is, that the base of the tumour never approaches nearer to the edges of the bones than from one to one line and a half, and never intrudes upon a suture or a fontanelle. The second, that the base of the well-defined swelling is encircled by a hard ring. By attention to the former, as also to the facts that the tumour is non-pulsating, does not move during deep respiration, crying, or coughing, cannot be lessened by pressure, and that the want of bone is an illusory sensation, cephalhæmatoma is to be distinguished from hernia cerebri. The presence of the "ring" is constant in *mature* cephalhæmatoma. Sometimes it is formed by the first day after birth, in other cases it is not complete in its whole circumference until several days after.* It begins to be formed so soon as the periosteum ceases to be separated from the bone, in consequence of the increase of the circumferential swelling of the tumour being arrested. 5thly. Valleix is probably correct in the main as regards the nature of this "bony pad, or cushion". It is due to ossification taking place in plasma-exudation, poured out by the periosteum in a state of irritation from the constant distension and pressure it suffers where it is separated from the bone by the extravasated fluid of the tumour. 6thly. As regards treatment, it may be stated that the latter may heal spontaneously, or inflammation or suppuration may ensue; or even caries of the underlying bone. Guided by the first fact, cold evaporating lotions may be applied to hasten the resorption of the extravasated blood. If in six or seven days no very marked diminution of it ensues, the hair is to be shaved from the whole surface of the swelling, and widely around its base: a puncture a quarter of an inch long is to be made by a lancet at a depending point of it, equable pressure to be applied by the fingers to expel as much of the contents as possible, and afterwards general pressure by a compress, the strapping and bandages to be maintained undisturbed for about six days. During this time attention must be directed to the patient and the tumour, so that the effects of the pressure, or the supervention of inflammation, &c., may be known.

In the same journal, Dr. Riecke details three cases of abscess occurring in the mastoid process, from the effects of exposure to cold during stormy weather in the early part of the same year. He throws out rather the whimsical hint, as it appears to us, that as the atmosphere at certain times is often loaded with "ozon," operating very excitingly upon the

* In the new edition of Bouchut's work (Maladies des Nouveaux-Nés), which we have just received, we find the following statement:—"M. Morel exhibited to the Society of Biology a case of cephalhæmatoma, in which the 'bourrelet osseux' existed only upon one side of the tumour." In default of further information, we assume that this is explainable by Professor Levy's views, regarding *maturity*.

respiratory passages, so a forcible draught of air rushing into the ear, charged with an irritating agent, may produce peculiar effects, such indeed as the production of the three cases he brings forward. Dr. Riecke, amongst other points he touches on, recommends the bichloride of mercury in acute hydrocephalus, and the application of the actual cautery in obstinate cases of umbilical hæmorrhage in the new born-child. We meet also with some observations on pertussis by Trousseau, by whom the affection is defined as a "specific and infectious bronchitis," and its treatment by emetics of the sulphate of copper, and afterwards by the powdered root of belladonna, recommended. The clinical experience of Mauthner von Mauthstein is so extensive, and his remarks always so practical in their character, that our readers may be glad to be informed that several pages of the journal are indebted to his pen. They are occupied by the subjects of laryngismus, eczema, and the effects of bloodletting. The dependence of the former upon abnormal states of the thymus gland is held to be negatived by experience, the disease being caused by a variety of circumstances, including epidemic influences, the form of the affection prevailing when Dr. Mauthner wrote, being symptomatic of a croupose catarrhal malady of the air-passages. In by far the generality of cases, the prognosis is favourable. In the less complicated forms, counter-irritation by croton oil, with the use of the tincture of rhubarb and the carbonate of soda, or the tincture of the malate of iron or cod-liver oil, with particular regard to the general hygiène of the child, is the most recommendable treatment. The author, although admitting his ordinary aversion to general bloodletting, notices those forms of pneumonia in which it is not only advisable to enforce it, but often absolutely necessary to do so to save the child.

We can do no more on this occasion than simply record the publication of the third, and, we assume, the concluding, portion of Dr. Bednar's treatise. The former two divisions of it (reviewed in vol. vii. p. 110, and vol. x. p. 1) led us to augur highly of its character when completed; and now we can safely say that it constitutes one of the best expositions of the morbid anatomy of children extant. It is rather more a book for reference than for continuous reading; but as the former, it is quite indispensable to any desirous of becoming acquainted with the detail of the important subject to which it relates.

W. H. Willshire.

Review XI.

Military Surgery; or, Experience of Field Practice in India during the years 1848 *and* 1849. By J. J. Cole, M.R.C.S.E., H.E.I.C.S., late Surgeon to the Auxiliary Forces during the war in the Punjaub.—*London*, 1852. 8vo, pp. 223.

The practice of the army-surgeon on the line of march or upon the field of battle consists in the adaptation of the established principles of surgery to the character of surrounding circumstances, and not in any material deviation from the laws laid down for the treatment of injuries in civil life. Consequently we find, without surprise, that a young surgeon carrying to India no other advantages than those afforded by the education at so excel-

lent a school of instruction as Guy's Hospital, is enabled to perform military duties of a peculiarly onerous and responsible nature with efficiency, judgment, and courage. He has not yet become a surgical martinet; he sees things with an eye uninfluenced by military routine, and he does not conceal ignorance by sheltering himself in the incomprehensibilities of "malingering."

Mr. Cole found himself, seven months after his arrival in India (November, 1847), in medical charge of Lieutenant Edwardes' army (18,000 strong), then encamped before Moultan, into which it had driven the rebel Moolraj and his troops. The wounded in the battles of Kuneyreh and Suddozam, in various skirmishes in the sieges of Moultan, during six days' cannonade from the Moolraj's guns, in the battle of Soorajkoond, in the siege of Luckie, passed through his hands. His health, which suffered from the hardships of the campaign, prevented his recording the cases which fell under his care so fully as he might otherwise have done; but no one can rise from the perusal of the volume without the conviction that it contains a faithful account of the scenes witnessed, and that the author exhibited no small amount of determination.

The style is somewhat dogmatical, and the language occasionally hard; a list of prescriptions might, with propriety, have been omitted; and the unqualified disapproval of the use of chloroform must be condemned. Mr. Cole has no right to "draw the attention of subordinate medical officers" (p. 89) to anæsthetics as highly pernicious agents, when they are now employed by most surgeons in civilized countries, after machinery or railway accidents quite as severe as those inflicted by missiles discharged by the explosion of gunpowder. No man, it is true, would administer chloroform to a patient in a state of extreme collapse; but that is no reason why the bottle should be "*left with the medical storekeeper, or be placed on a high shelf in his warehouse with the stopper from it to keep it cool.*" (p. 89.) Every medical man in the public services should be provided with chloroform as fresh and as pure as possible, for there is many a case in which an operation performed without pain will save the life of a patient unable to sustain the shock of more suffering without dying.

We could have wished a full account of the construction of hospitals, and of the different means adopted to keep this singular force under Lieutenant Edwardes' command in health; but the work is purely surgical, and treats only of injuries to the soldier upon the field of battle. Speaking of the effects of a cannon-ball, he says:

"For the first five or six hundred yards it grinds to powder, and destroys everything. If the wounded person be a European, the skin looks more or less livid; if an Indian, blacker than natural. The tissues are torn through, and hang down in shreds; small portions of the muscles, veins, arteries, and other structures, come away when you pinch them with the finger and thumb, and the exposed surface of the truncated bone is almost ground to powder. . . . These wounds are not, however, as fatal as the injury the same shot is able to produce when it has travelled four or five hundred yards further, or when, perhaps, it has nearly reached its goal; then it is that it occasions the most severe lesions which can befal the extremities of man. It tears its way, apparently with some little difficulty, through the structures, and hangs in the wound long enough to impart its destructive influence to the adjacent tissues. The integuments are extensively lacerated above and below; the muscles are dreadfully torn, and separated from their attachments, and their

interspaces are filled with coagulated blood. Although it is true," continues the author, "that many gun-shot wounds do not bleed at all, yet it is better to view all wounds of this nature as hæmorrhagic. The surgeon sees the patient with the body cold, and the pulse scarcely perceptible; but let him go to the spot where the round shot strikes him, and the bleeding is found sufficient to call forth every skill." (pp. 6—9.)

Upon this point public attention has been especially directed by Mr. Guthrie. Wounds of large veins are deemed more dangerous than those of arteries.

We trust that for the future it will not be necessary even to refute the nonsense about the injurious effects of air put in motion by the cannon-ball.

Mr. Cole is an advocate for immediate amputation, but he does not indicate the precise period for performing the operation so clearly as does Mr. Guthrie in his work on Gun-shot Wounds, nor does he point out well the danger attending immediate collapse. "During the course of the Peninsular war, the success of amputations," says Mr. Guthrie, "performed on the field of battle became so notorious, even among the soldiery, that the anxiety expressed by them to have these operations executed with as little delay as possible, has frequently become prejudicial, the sufferer not having had time to recover from the shock of the injury and to approach even in some degree the state of health."—(*On Gun-shot Wounds.*)

Mr. Cole recommends that gun-shot wounds of the chest should not be closed, but rather left open for the escape of fluids, in opposition to the course of treatment suggested by both Larrey and Hennen. He also thinks it right to remove a small portion of the rib with a Hey's saw, to secure an intercostal artery, when the hæmorrhage cannot be restrained by the usual means. The surgeon who would undertake this operation must be a bold man; and it is worthy of remark, that such a measure is never thought of in ordinary cases of fractured ribs, when the vessels here spoken of must often be torn. Fatal hæmorrhage into the cavity of the chest would in all probability be from some other source; either from the vena azygos, or from the large vessels about the heart, of which accident there are cases upon record. Hennen observes, that in some irritable habits, where the bony covering is removed, the hæmorrhage from the intercostal arteries is much more profuse than could be supposed from their size; yet he adds, "I have never met with a case requiring the tenaculum." Plenck, it is true, carried a needle round the rib of an injured intercostal artery, and putting a tent under it, tightened the ligature so as to compress the vessel; Theden, the Prussian surgeon-general, found the plan, upon trial, followed by fatal consequences, as any reasonable man might have anticipated.

We are glad to see that no undue stress is laid on putting the patient upon the injured side; a piece of advice often impracticable and useless.

An European who surveyed, for the first time, the small-handled, apparently ill-balanced, but sharp-edged sword of the Sikh or the Belooch, would scarcely fancy it capable of inflicting such severe wounds in the hands of the experienced warrior as those recorded by all observers. The British soldier soon ceased to regard with ridicule the small dark-coloured,

yet active native, who, under the combined influence of religious enthusiasm and of opium, danced along the line alone, muttering some chaunt, wounding those whom he could reach, and prepared to die upon the field. The weapon is not used for striking, and but rarely for thrusting; it is drawn across the object with such address, that it cuts down to the bone even in such a well-covered limb as the thigh. Mr. Cole relates the case of a soldier who was most effectually scalped, "a portion of the integument, three inches by two and a half, being clean cut off." (Case lxxx.) In other instances the bone was divided. In case lxxxii., the sword in its course "cut off the ala of the right nostril, shaved the malar bone, severed the lobe of the right ear, grazed the ramus of the jaw exposing the teeth, turned into the neck, and terminated in front at the sternal end of the clavicle, behind at the spinous processes of the cervical vertebræ, laying three of them bare." In the treatment of such wounds all morbid fear respecting the use of sutures must be set aside—the edges must be effectually approximated till lymph is effused.

We think some of Mr. Cole's methods of treatment severe. In wounds of the tongue attended by bleeding, he recommends—

"Pinch the tongue above the wound with a flat pair of forceps until the bleeding ceases; quickly sponge out the wound, dry it, and apply nitrate of silver freely over its whole surface: now allow the tongue to return into the mouth, to be again drawn out and treated as before, if hæmorrhage returns. A ligature will occasionally suffice, especially when the wound is near the lip. Often, however, neither ligature, nitrate of silver, nor any other styptic, will succeed; nothing but the actual cautery—the red-hot iron itself—will do!" (p. 160.)

The tongue goes no further back than the os hyoides, which can be easily drawn forwards to the front of the mouth; we have witnessed the tying of cut arteries of the tongue without difficulty, after the extirpation of one-half of the organ. Dangerous hæmorrhage from such wounds is rare.

Again, in speaking of opening the temporal artery in the treatment of iritis, why should *caustic* be applied to the wound? Such a proceeding is both unnecessary and painful.

The author approves of the application of a ligature to the femoral artery after wound to the popliteal (p. 168); and he has in his possession, no doubt, some successful cases in support of the plan here approved. He does not, however, clearly state whether the treatment has been positively satisfactory in his hand—a point of much practical interest now that great importance is attached to the *fiat* that surgeons should in all cases cut down to the wounded artery at the spot injured, and put a ligature above and below the aperture. That such, when readily within our power, is the proper course of treatment, no one can for a moment doubt; but it might be a question whether amputation would not be preferable to the operation of putting a ligature upon the popliteal in a limb much bruised by the passage of a bullet, with torn muscles, with parts displaced and disfigured by extravasated blood, and when the immediate seat and absolute extent of the injury were doubtful. A well-authenticated case, to show that under such circumstances ligature of the femoral may be successful, would be, at the present moment, valuable and acceptable.

The work shows talent, and is both truthful and original. Mr. Cole evidently had the welfare of the soldier at heart. The reader will find in it much that is interesting and useful, and the blemishes can be easily rectified in a second edition.

Holmes Coote.

Review XII.

Mémoires de l'Académie Nationale de Médecine. Tome XVI. pp. 818.—*Paris*, 1852.

Of the great part which Academical Transactions have played in the advancement of medical knowledge, there can be but one opinion, and the beneficial tendency of their operation is as great now as at any period of their history. For, if at an early stage of this they formed the principal mode of intercommunication of learned Europe, at the present time they rescue many a meritorious production from the whirlpool of periodical medical literature, which, while it swallows up all within its reach, by its very multiplicity renders the escape from notice easy, and the process of search difficult. In the Transactions of Academies and Societies, the author's production appears with the dignity of an *imprimatur* (sometimes too easily granted, it must be admitted), unsurrounded by the heterogeneous mass of matter which encumbers most of our modern periodicals, and in a form that is comparatively easy of reference, and certain of preservation.

Among these Transactions, those of the French Academy of Medicine scarcely, in our opinion, holds so high a place as might have been expected from the official publication of so renowned a body; and certainly, whether viewed in relation to their practical utility, scientific value, or the important improvements they have heralded to the world, are considerably inferior to those of our Medico-Chirurgical Society. It is to be observed, however, that besides its Transactions, the Academy publishes an official *Bulletin* of its weekly meetings, in which detailed and corrected reports of all papers read, and of the reports and discussions these give rise to, are contained. The reports, indeed, usually confided to men of great eminence in the branch of knowledge to which the communication relates, are often of high value and interest; and it has not unfrequently happened that papers of very poor pretensions have given rise to most admirable reports and animated discussions, in which the leading men have taken their full share. This is a practice we think might be beneficially imitated in this country. At present we have at the Royal Society reports without discussions, and at the Medico-Chirurgical Society discussions without reports. The consequence is, that while in the former body the grounds of the report are insufficiently sifted, in the latter the discussion is often of the most trivial and conversational character, if indeed it can be called discussion at all. What else, indeed, could be expected from *extempore* remarks on a paper read in an abbreviated form, and often only imperfectly heard. If, however, it were confided to the leisurely consideration of a well qualified reporter, its merits and demerits might be thoroughly examined in relation to the present state of knowledge upon the subject. The conclusions the reporter came to would form a subject of profitable

discussion, in which the most eminent members of the Society would probably join, especially if their consideration were adjourned, as is often the case at Paris, to the meeting after that at which they had been stated. The inducement to join in such discussion, and the utility derivable from it, would be much enhanced if the Society published its *Bulletin*, like the French Academy.

The present volume of the Memoirs is a good average one, and we proceed, without further preamble, to place an account of its contents before our readers. The introductory portion, consisting of some historical particulars concerning the celebrated Paris Academy of Surgery, and its no less celebrated secretary, Anthony Louis, and an *éloge* upon Richerand, from the pen of M. Dubois (d'Amiens), the secretary, need not detain us farther than to remark that if the mode in which the French signalize the departure of their great men is somewhat too theatrical, we, for our parts, allow ours to go from amongst us with too much indifference.

1. The first paper is a report, by M. Gaultier de Claubry, upon the "*Epidemics which prevailed in France during* 1848 *and* 1849." In noticing M. Gaultier's former reports upon this subject (vol. ix. p. 51), we described an admirable provision made in France by the appointment of certain medical officers, called "Physicians for Epidemics," whose duty consisted in watching, and reporting upon, the different epidemic visitations that may occur. Like many other institutions, however, this seems to look better on paper than to work in reality; for from these medical officers, scattered all over France, only thirteen reports, relating to nine departments, were received in 1848, and but twenty-two in 1849. The political circumstances of the country in the former year may perhaps account for the paucity; and in the latter year M. Gaultier was precluded from reporting on the cholera, for the consideration of all facts relating to which a special commission, with M. Guerin as reporter, has been appointed. There is little to require our notice in either of the two reports. *Typhoid fever*, as is always the case in France, constituted by far the most prevalent disease, and predisposing causes of the most opposite kinds are assigned by the physician-reporters. M. Poggioli, an army surgeon, gives an interesting account of an epidemic of the so-called *cerebro-spinal meningitis* which prevailed in the garrison of St. Etienne (Loire) from June to October, 1848. The garrison consisted of two squadrons of dragoons, all old soldiers, of 1100 men of the 13th regiment of light infantry, almost all being old Algerian soldiers, and of a similar number of the 22nd regiment, composed chiefly of new conscripts. This last regiment was well lodged and fed, and under very mild discipline. The barracks of the other two regiments were in far less favourable hygienic conditions. Nevertheless the 22nd had 107 men seized, of whom 32 died; and when no new cases appeared, then came the turn of the 13th regiment, in which, however, five only were attacked, of whom two died. Among the dragoons only one case occurred, which proved fatal. Between the 11th June and 3rd October, of 25 patients, 23 died. M. Poggioli was now called in, and instituted free cupping at the nucha, rubbing in over the scarifications muriate of morphia, with extract of belladonna and mercurial ointment. Employed at the commencement, this treatment seemed to cut short the disease; but if the case had made some progress, it failed to do so, and active antiphlogistic and revulsive treat-

ment was resorted to. Tincture of cantharides was administered internally. After this treatment was put into force the alarming mortality much diminished. The etiology of the disease, on this occasion, was just as obscure as on so many others. Epidemics of *dysentery* were remarkably prevalent in 1849, occurring from August to October, after the excessive heats of a burning summer; hygienic conditions seeming to have exerted no appreciable influence on the production, duration, or extension of the epidemics.

2. *M. Bouchardat on the Nature and Treatment of Diabetes Mellitus, or Glucosuria.*—This paper gives a connected account of M. Bouchardat's researches upon diabetes, which have now extended over twenty years, and have been reported from time to time in his 'Annuaire de Thérapeutique.' With a tenacity not unusual in science, he holds firmly to his own opinions, and appears scarcely able fully to recognise the facts noted by others. In spite of the numerous observations which are opposed to his view, and which, to say the least, prove it to be incomplete and partial, Bouchardat arranges his opinions with as much confidence as if their accuracy were not almost universally denied. However, we will allow him to speak for himself, and to present in one connected view his matured creed.

Bouchardat still believes that the normal digestion of starchy substances is the starting point in this discussion. Diabetic patients do not digest these as persons in health do, and they suffer from thirst in proportion to the amount of farinaceous matter they consume. The quantity of fluid requisite to assuage thirst is exactly equal to that required to aid the action of *diastase* on starch. In the stomach of a healthy man, two or three hours after a meal, very little glucose will be found; in the stomach of a diabetic patient at the same time, a large quantity is present. In persons in health, aliments for the most part pass into the small intestines before they undergo solution, and even there this process is far less rapid than in diabetic patients. In the latter, diastase is secreted in the stomach, and feculent aliments are, through the intermedium of the water which their thirst compels them to drink, by it converted into glucose, which is absorbed by the numerous venous ramifications, and transported into the blood, to be excreted in the urine. This rapid absorption causes a feeling of emptiness, which induces the patient to take more food, and with no effect, as regards his nutrition, if it still consists of farinaceous substances. M. Bouchardat does not consider that the discovery of sugar in the liver by Bernard, at all militates against these views; believing, in spite of the careful observations of Lehmann on the blood of the vena portæ and hepatic vein, that the liver does not generate the sugar, but retains it in its substance, whencesoever derived, to pour it into the blood. When the food employed can only furnish very little sugar, as in the case of meat (inosite), only the small quantity retained by the liver is to be found, it being no longer detectable in the blood of even diabetic patients. When, on the other hand, feculent food is given in glucosuria, the moderating power of the liver no longer suffices, and large masses of sugar are poured into the circulation, to be eliminated by the kidneys—experiment having shown that such elimination always takes place when a larger quantity than 30 grains are present in the blood.

The diminution of temperature by 1° or 2° Cent., always observed in

glucosuria, and probably due to the refrigerating effects of cold drinks, and to the waste of caloric in converting starch into glucose, doubtless is one of the causes which render the destruction of the glucose in the blood slow and incomplete. A diminution of the alkalescence of the blood would have a similar operation; and a knowledge of the fact has led in certain cases of the disease to the beneficial administration of alkalies. The theory of M. Mialhe, however, which explains glucosuria exclusively by a vicious assimilation, due to the diminished alkalescence of the economy, is completely at fault. M. Bouchardat has frequently noted remarkable aberrations of the nervous system in diabetic patients; but he is by no means disposed to conclude, from the induction of sugar in the urine by Bernard's experiment of irritating the floor of the fourth ventricle, that this system exerts a primary influence in the genesis of glucosuria. The presence of sugar may, in fact, be determined by various influences, and it is possible that this irritation of the fourth ventricle operates through the diminution of temperature it gives rise to. „The diabetic patient, deprived of sugar or feculents, no longer produces glucose; while the urine of an animal, similarly deprived, in whom this experiment is performed, exhibits it. Moreover, the amaurosis met with in diabetic patients, disappears under suitable hygienic measures, without any special action upon the nervous system being resorted to. Still it is to be remembered that in bad cases of glucosuria a decided affection of the *medulla spinalis*, attended with paraplegia, exists; but although, in some cases, this possibly may be a primary cause of the disease, in others it is inadmissible. Indeed, in M. Bouchardat's opinion, multiple causes may give rise to glucose, and any theory founded on an exclusive one is defective. His own statement that feculent substances are differently digested by the diabetic, and that the thirst and amount of glucose found in the urine are proportionate to the quantity of these consumed, is merely, he says, the result of observations that every one may verify; and he considers that it is an injudicious step to call upon him to enter upon the difficult question of why diabetic patients digest differently to persons in health. Nevertheless, he offers the following explanation.

"The most active ferment for the digestion of feculent aliments is the diastase found in the pancreatic juice; but almost all the other digestive fluids contain an albuminoid principle, which, in lesser degree, possesses a similar solvent action, that under the influence of different causes may become augmented in power. Thus, while in the dog and the pigeon the pancreatic juice is almost exclusively charged with the solution of feculent bodies, if we tie the duct of the pancreas in the one animal, and extirpate the gland in the other, the albuminoid matters of the gastric and intestinal juices gradually assume the same solvent powers as the diastase of the pancreatic juice possessed. If, in the case of diabetes, we assume atrophy or organic disease of the pancreas, the albuminoid matter of the gastric juice would gradually assume the solvent power, and the digestion of feculents be achieved in the stomach in place of the intestines." In answer to this hypothesis, it may be stated, that while in some cases obvious changes in the pancreas and ducts have been observed, these have not been seen in the great majority. Again, in some patients there may exist a narrowness of the pyloric orifice, and a diminished activity in the contractile fibres of the stomach, which permits the dilatation of its cavity

and M. Bouchardat has always found the stomach much more developed in glucosuria than in health. Moreover, such persons almost always have an excessive liking for bread and other feculent aliments. The stomach, thus habituated to this long delay of so much feculent aliment, acquires in the modifications of its juices the power of dissolving it. This hypothesis explains the intense thirst, inasmuch as the feculent aliments require for their digestion from seven to ten times their weight of water, which the gastric juice is quite unable to furnish; as it also explains why, during the suspension of digestion in the dying, or in those suffering from fever, the sugar may disappear from the urine of the diabetic.

Treatment of Diabetes.—By the plan which M. Bouchardat now recommends to our notice, in its full detail, he declares that he can cure the majority of cases of diabetes—his test of cure being not only present removal of the sugar from the urine, but the ability of the patient to employ fæculent aliments without its reproduction. He, however, requires the intelligent co-operation of his patient, and, above all, the frequent testing of the urine, by the patient himself,* as a means of ascertaining progress and guarding against relapse. The means chiefly to be relied upon are those of a hygienic character; and, at all events, the power of these should be exclusively ascertained at first, before resorting to any medicinal agents.

1. *Diet.* As long as the urine exhibits sugar, all feculent and saccharine aliments must be entirely excluded; but the patient need not be confined to what is called an exclusively flesh diet, although this, when not repugnant to him, is the best. Every description of meat, dressed with the usual sauces and seasonings, (to the exclusion of flour, however,) may be employed; and for those who can get over the prejudice against it, the flesh of *carnivorous* animals, M. Bouchardat says, is best. By proper management (and what cannot a French cook do?) that of the cat or fox becomes a highly relishing viand. Several poor patients, who otherwise would have been unable to procure a flesh diet, have resorted to this means with advantage. Fish, in all its numerous varieties, forms a valuable resource for both rich and poor, and may be eaten with abundance of oil and a moderate quantity of vinegar. Eggs, again, so susceptible of varied modes of preparation, are excellent; and although milk is forbidden, good fresh cream and all kinds of cheese are allowed. Except in extreme cases, green vegetables and salads, although they contain some sugar, starch, or gum, may be taken in moderate quantities; but abundance of oil, or the yolk of eggs, should be conjoined. For such patients who cannot well overcome their liking for bread and other feculents, M. Bouchardat has, during the last ten years, had prepared a bread of flour containing 70 per cent. of gluten.

As the prohibited feculent and saccharine bodies belong to the respiratory group of alimentary substances, we have to choose others from the same group; and those best calculated to supply their places are fatty bodies and alcoholic drinks. Among the latter, Bordeaux wine occupies a prominent place, as much as from one to two *litres* (from two to four pints) being admissible *per diem*, which, at 10 per cent. of alcohol, would supply about 150 *grammes* ($2\frac{3}{4}$ oz.) of this substance in the 24 hours. Fatty bodies

* For indicating the presence of sugar in the urine, he employs lime water, and believes it to be a safer test than the copper solution; for the quantitative determination he employs exclusively Biot's polarizing apparatus.

must not be given too exclusively, lest they excite disgust, but mingled with other aliments, from 150 to 200 *grammes* being required, in addition to the alcohol. Beer is objectionable, from containing dextrine. Coffee, drunk without milk or sugar, and to which a little rum, cream, or brandy may be added, is a good drink. To relieve thirst, Seltzer, Spa, Vichy, or soda water may be taken; but acid drinks, so keenly desired by the patients, are very objectionable. The patient should always eat and drink in moderate quantities, slowly masticating his food. This practice tends to the relief of the attendant dyspepsia, and to assist the distended stomach to return to its normal dimensions. A flannel bandage applied around the epigastrium contributes to the same end.

2. *Clothing.*—As chills operate very injuriously on these patients, warm flannel clothing forms a valuable protective agent, and beneficially excites the languishing functions of the skin. Indeed, some medicinal agents are of no avail unless aided by complete flannel clothing, which maintains diaphoresis. General frictions are very useful, and a moist warmth of the feet should be maintained.

3. *Exercise.*—To recommend this indiscriminately would be injudicious, for many patients are too feeble to undertake it. But when their strength has become somewhat recruited by regimen, walking, gymnastics, agricultural labour, &c., much expedite the cure, and are found, as recovery is approaching, to enable the feculent aliments to become utilized by the system.

4. *Pharmaceutical Agents.*—M. Bouchardat entertains a high opinion of the utility of *carbonate of ammonia* (from 5 to 15 *grammes*—77 to 230 grains—in the 24 hours), providing flannel clothing be worn. Other *alkalies* suffice for slighter cases, when the urine contains uric acid as well as glucose. Employed consentaneously with out-of-door exercise, they seem to exert great influence in preventing the reappearance of sugar in the urine, when feculent aliments are resumed. *Opiates*, if given alone, are mere palliatives; but when conjoined with other remedies, and in moderate doses, so as to act on the skin, they are very valuable. M. Bouchardat sometimes prescribes Dover's powder, but prefers the old theriaca before all other preparations, without defending the absurd complexity of its composition.

In severe cases of glucosuria, then, diet, exercise, and flannel clothing constitute the basis of treatment, carbonate of ammonia and opiates best aiding their action. Other remedies have their occasional uses, such as iron, tonics, chloride of sodium, and antiscorbutic plants. M. Bouchardat often employs emetics at the commencement, and endeavours to modify the disturbed functions of the liver by aperients, of which, ox-gall with rhubarb are the best.

Circumstances influencing the effects of Treatment.—Foremost among the *favourable* indications in a case is the *rapid return of the urine to a normal state*, which may take place in from 24 to 48 hours after the feculents have been excluded. The *recent date of the affection* is another highly favourable circumstance; and because it is so, M. Bouchardat urges testing the urine whenever the slightest suspicion can be held, and for the detection of relapses, which are frequent and insidious. Other favourable circumstances are the retention of considerable *embonpoint*, the easy circumstances of the patient, and his being in possession of great perseverance.

The *unfavourable* circumstances are the reverse of the above; but negligence is still worse than poverty, as the poor man has some resources. The treatment of the case is usually ill managed in *hospitals*, owing to the vitiated air, the absence of exercise, the sameness of diet, and the insufficiency of surveillance. The existence of a great *appetite* is a common and not unfavourable circumstance, requiring only moderation in its gratification, at meals not too far separated. *Want of appetite* is a far more unfavourable sign, which should be actively combated. M. Bouchardat has found small doses of rhubarb, and exercise in the open air, of advantage. *Obstinate constipation*, resisting the most varied purgatives, is a bad complication, indicating disease of long duration, which has produced important modifications in the condition of the alimentary canal. Fatty substances, combined with matters which leave residue, as spinach and gluten-bread with bran, are here indicated. *Cold and damp air* is unfavourable to diabetic patients; but M. Bouchardat has had patients from Algeria, and has not derived advantage from sending others to Italy. M. Bouchardat agrees with Dr. Prout in considering the appearance of *albumen* in the urine, which is often met with, as an unfavourable occurrence. The prognosis of saccharine albuminuria is not so serious as is that of simple chronic albuminuria. The frequency of the occurrence of *phthisis* in cases of glucosuria is familiarly known. In all the autopsies the author has made, when the patient has not been cut off by an intercurrent affection, tubercles have been found in the lungs; and he feels convinced that many cases of phthisis have had their origin in a glucosuria that has been overlooked, and which might have easily been removed. In severe and old cases of glucosuria, *vision* is always found more or less enfeebled; but in most cases, when not of old date, as the condition of the patient has improved under appropriate regimen, this amaurosis has subsided. When, indeed, this is not the case, the prognosis of the glucosuria is serious; and it will often be found complicated with albuminuria. *Impotence*, more or less decided, is another effect of glucosuria; but in young subjects the generative functions resume their power when the original disease is rationally treated. Glucosuria may occur at any *age*, from infancy to senility; M. Bouchardat having met with most cases between forty and fifty. He has met with none between eighteen and twenty-five. Old age does not constitute an obstacle to cure; but so difficult is it to watch over children, that the author is not aware of a sustained cure prior to fifteen years of age. He has met with more male than female patients.

The two next memoirs do not call for notice. One of them consists in the minute relation of a successful case of "Amputation at the Hip-joint" for exostosis of the femur, by M. Henot; and the other is an essay on "Cervical Adenitis," as observed in the French military hospitals, by M. H. Larrey, a summary of which we furnished (vol. vi. 260), when the paper was reported upon to the Academy by M. Grisolle.

The fifth memoir is from the able pen of M. Gosselin, and is entitled, "*Researches on the Synovial Cysts of the Hand and Wrist.*" In it he relates the results of his anatomical researches, made in the attempt at clearing up some of the confusion and contradictions that have prevailed in the description of these tumours. Confounded formerly under the general name of *ganglion*, these swellings have been, of late, distinguished by most writers accordingly as they have occupied the track of the flexor

tendons of the palm, or the extensors at the back of the wrists. The distinction is, indeed, an essential one, for in the former case (*dropsical cysts*), an entire synovial bursa is involved, while in the latter (*partial or ganglionary cysts*), a very circumscribed portion of the synovial membrane is concerned. The object of this paper is to determine the anatomical limits and connexions of the first of these, and the mode of formation of the second.

1. *Dropsical Cysts of the Palm.*—Authors have varied much in the descriptions they have given of the direction and number of the synovial membranes accompanying the flexor tendons; and the dissection of more than sixty hands, in both children and adults, enables M. Gosselin to account for this discrepancy by the varieties of distribution which actually prevail. Amidst these there is, however, a regular disposition observed in the majority of cases, and met with in the fœtus, children, and women not accustomed to manual labour. There are two synovial membranes, which, taking their origin a little above the annular ligament, are continued, the one to the second phalanx of the thumb, and the other to the second phalanx of the little finger. The first of these is reflected from the level of the carpus around the tendon of the flexor proprius of the thumb. The internal one is reflected on to the tendons of the flexors of the little and ring fingers, extending to a much less length along the latter. The tendons of the index and medius fingers are unprovided with these bursæ, and may be separated without opening either the outer or inner synovial cavity. Normally, these two cavities do not communicate, but an occasional variety is met with in which they do. A more frequent variety consists in the presence of a supernumerary sac, placed between the two others, above the annular ligament, and not unfrequently communicating with one of them, especially the internal. In the course of the prolongation of the internal cavity it normally undergoes a narrowing at about midway of its course, and a not unfrequent variety is found in its becoming obliterated at this point. As a fourth variety may be noticed the presence of small, isolated, synovial sheaths, distinct from the others, formed from the cellular tissue of the superficial tendons. Thus, normally, there are two synovial cavities in this locality; and when authors have described more, they have been alluding to some of these varieties. We see also why, as a result of chronic inflammation, the little and ring fingers are those especially liable to become contracted.

2. *Ganglionary or Partial Synovial Cysts.*—M. Gosselin has nothing new to communicate respecting the symptoms or treatment of these ganglions, so often met with at the back of the wrist, his object being merely to draw attention to their mode of origin. After adverting to the different explanations of this, hazarded by prior writers, he states that he has been led to offer a new one, by having observed, in a great number of radio-carpal joints he has examined, immediately beneath the synovial membrane, behind the semilunar and scaphoid bones, certain whitish or greyish corpuscles, varying in size from that of a millet-seed to that of a pea. Some of these slightly project into the articulation, but, in most cases, concealed in the sub-synovial cellular tissue, they are only brought into view by the careful removal of the synovial membrane, to which they somewhat firmly adhere. Usually they do not communicate with the joints, no orifice being discernible; and their contents, which resemble

those of ganglions, only issue out upon incision. These *sub-synovial* bodies are also met with in the medio-carpal joints, but much more rarely. Whether viewed as to their *locale* or their contents, these bodies must be regarded as exhibiting the earliest stage of ganglionary formations; and the frequency of their existence, and regularity of their position, lead to the inference that they are modifications of a natural disposition. Additional researches have brought M. Gosselin to the conclusion, that all the articular synovial membranes (and especially those of the wrist) are provided with prolongations, or *culs de sac,* which may be called *synoviparous crypts, or follicles,* and that the obliteration of the orifice of these crypts gives rise to the accumulation of synovia. The crypts are, indeed, but one of the means employed for extending the synovial secreting surface; and in some instances they communicate with the general cavity by broad, and in others by very minute, openings, the latter being liable to this obstruction. Velpeau and the Webers have, indeed, adverted to these synovial prolongations, but they have not generalized their descriptions, or made the pathological application. These crypts are to be found in all the principal articulations; and in the horse these follicles and their communicating orifices are found on a much larger scale. Taking this general view of them, the conclusion becomes irresistible, that these depressions of the synovial membranes are means for the enlargement of surface analogous to the single follicles of the skin and mucous membranes. The reason why the orifices of these follicles of the wrist-joint should so especially become obliterated, and give rise to ganglions, is not to be ascertained any more than that of the greater frequency of sebaceous cysts in certain regions of the skin. The sub-synovial bodies thus produced may remain stationary, their outward progress being opposed by the aponeuroses which surround them; or they may increase towards the cavity of the joint, into which they may at last burst. If the aponeurotic resistance is less decided, or in part destroyed, the bodies make their way towards the surface under the form of *ganglions.* M. Gosselin has been unable to observe any of the synoviparous follicles in the sheaths and bursæ of the tendons; and it is therefore a highly important fact, in a practical point of view, that the ganglions are in communication, not with these sheaths, but with the joint itself, into which, indeed, they sometimes, at an advanced period, discharge themselves.

The next essay is an important practical paper by M. Hutin, chief surgeon to the Invalides, upon the "*Necessity of extracting foreign bodies and splinters, in the treatment of Gun-shot Wounds.*" In a celebrated discussion which took place at the Academy after the events of June, 1848 (Brit. & For. Med.-Chir. Rev., ii. 261, 546), great discrepancy of opinion prevailed among distinguished surgeons as to the rule of practice to be followed in this emergency. The present paper, based upon its author's fourteen years' experience in Algerian warfare, and five years' observation of the results of treatment among more than 4000 inhabitants of the Invalides, is an energetic protest against the temporizing practice advocated by some surgeons. It scarcely calls for or admits of analysis; and we may content ourselves with saying, that the advocates for the prompt removal of foreign bodies will find in M. Hutin a most able coadjutor, speaking with great authority, in consequence of the opportunity he has had at the Invalides,

of contemplating the results of the most opposite lines of practice. In relation to this subject we cannot refrain from noticing the fact, that while by the events of 1848 surgical science received illustration on several important points, those of 1851 have not been allowed to contribute to this end. No cases, statistical results, or clinical lectures, have appeared in any of the medical journals; and a reader relying upon them alone for information, would remain in absolute ignorance that occurrences attended with so much suffering and slaughter had ever taken place. Repression has invaded even the realms of science.

The last memoir in the present volume is a prize essay by M. Broca, Prosector of the Faculty of Medicine, having for its subject the "*Pathological Anatomy of Cancer*," and occupying nearly 400 pages. Of so lengthened a production, a minute analysis with the space we have at command is out of the question; and we will only attempt to furnish an outline of the conclusions the researches of M. Broca have led him to arrive at; and especially of such of them as differ somewhat from those of other observers. While entertaining a very high opinion of the value of the revelations of the microscope, he believes that real practical utility can only be attained by conjoining laborious clinical research with its employment; and to the neglect of such association he attributes the errors and reveries of certain of the German microscopists.

M. Broca considers that the *lactescent juice* which every cancerous tumour yields to pressure or scraping, sufficiently characterizes the disease, in the great bulk of cases, even to the naked eye. Any tumour yielding no juice at all, or only a *serous* juice, may be at once set down as non-cancerous. (?) It is true, lactescency may result from other causes, as purulent, lacteal, and tubercular infiltration, &c., and in certain of these cases may require the intervention of the microscope to distinguish the nuclei and nucleated cells, which constitute the essential feature of cancer. The microscopic appearances are given in full detail; but with these the writings of Professor Bennett (whose researches receive from M. Broca well-deserved acknowledgment) and others, have rendered our readers familiar; and we will only quote one of the concluding passages of the description.

"It is thus seen how varied are the forms and mode of existence of cells in cancer. We find in cancer, and in it alone, every disposition which the elementary vesicles are capable of assuming in organized beings. Every imaginable type, every possible mode of production and multiplication—isolated development, endogenous generation, fissiparity, are found combinedly or separately in this tissue." (p. 482.)

M. Broca first considers cancer as a tumour in the abstract, and then as a disease affecting various parts of the economy. Its principal forms are next described, the paper terminating with an account of the characteristics of *pseudo-cancer*. In treating of the *local evolution of cancer*, he asserts that Vogel's statement that fibres are first organized in the blastema in which cancer-cells are afterwards deposited, must have arisen from his having mistaken for cancerous deposits the lymph which is effused in the immediate vicinity of the tumour, as a consequence of the irritation it excites. In early cancer M. Broca has usually found a large proportion of free nuclei, while the nucleated cells that are present are very much

smaller than they are at a more advanced stage of the disease. He totally denies the possibility of healthy tissue undergoing *degeneration* or *transformation* into cancerous, *substitution* supervening upon atrophy (as in the so-called fatty degeneration of muscle) being really the change which does take place. In like manner he cannot admit that a homœomorphous or innocent growth is ever converted into a heteromorphous or malignant one. He points out the utter absence of evidence of such change, that can bear the scrutiny of modern means of investigation. A supposed homœomorphous growth must never be removed on the pretext that it may degenerate. It has the same, but no more, risk of becoming a seat of the deposit of cancer, as any of the normal tissues. Cancer invariably *increases;* and cases, in which a *partial* diminution of the size of the tumour takes place, must be regarded as examples of resorption of homœomorphous elements, which have been deposited amidst the heteromorphous elements. As to the *entire disappearance* of a true cancerous growth, not only can no clinical fact of sufficient certainty be adduced in its favour; but the results of histological investigation do not encourage the admission of the possibility. M. Broca bears testimony to the faithfulness of Bennett's description of the fatty and granular alteration sometimes observed in the cells; but he cannot admit that these should be regarded as abortive or retrograde cells, or as tending to show the possibility of the spontaneous cure of cancer.

The increased deposit of cancer-cells is attended with the *propagation* of the tumour at its periphery. Its progress is for a while delayed by a greyish or yellowish infiltration of lymph, which surrounds the tumour; but this is invaded by the increasing deposit, which gradually insinuates itself into the interstices of surrounding tissues, compressing and causing the atrophy of these, and eventually occupying their places—not by transformation, but substitution. The looser and the more cellular the tissue, the more easily does the new deposit gain admission, while where but few interstices prevail, the slower is the progress of the cancer in that direction. As the cancer advances, too, its substance undergoes *ramollissement,* which M. Broca looks upon as a true phasis of its evolution, and sign of the increased activity of the specific element of the disease, and not as an internal gangrene with Hodgkin and Carswell, or a process of decay with Walshe. This real *ramollissement,* which affects a tumour at first hard, is to be distinguished from the *apparent* ramollissement due to the deposit of certain portions of cancer which are soft or diffluent from the beginning.

The *vascularity* of cancer is usually proportioned to its softness, being slight in schirrus and considerable in encephaloid; proportioned, in fact, to the amount of the specific elements of the disease, the nuclei and cells. The circulation presents great analogy to the cerebral circulation, arteries, and veins, ramifying upon the surface, while capillaries penetrate the substance. The middle coat of *arteries* may long resist the progress of cancer, the compression caused by which may induce partial obliteration of their calibre. Where this is not the case, extensive external hæmorrhage may follow the successful invasion of the disease, or an apoplectic effusion may take place into the substance of the tumour—the effused blood undergoing changes very similar to those observed when it is effused in normal struc-

tures. Sometimes such effusions are multiple and intercommunicate, the blood not coagulating, and receiving an impulse from adjoining vessels. This is often the case in encephaloid disease of bone, and has been mistaken for *erectile tumours* of that tissue. As far as M. Broca's experience has gone, all such so-called erectile tumours of bone are really examples of this *hæmatode cancer*. The thin and unresisting structure of the walls of *veins* soon yield to the invasion of cancer, but their contents are not effused into the substance of the cancer. Veins of small size may become quite obliterated, but those of larger calibre are either opened into or plugged up with a cancerous deposit, more or less of which may become detached, and gain admission into the circulation. *Inflammation* of cancer is of far seldomer occurrence than from its richness in capillaries might have been expected; and inflammation occurring around the cancer has often been confounded with that of its substance. It is an error to consider *ramollissement* of cancer as one of its results. Several of the alleged cases of *suppuration* of cancer will not bear examination, that quoted by Walshe from Levêque-Lasource among them. But rare as such cases are, M. Broca has met with an indubitable one occurring in cancer of the lungs. He has also had three cases of partial *gangrene* of cancer, and has met with about thirty others recorded, the great bulk being examples of encephaloid.

Treating of cancer as a disease, M. Broca states he has only met with three cases in which the *primary tumour* was not a *solitary* one, and M. Lebert has communicated another to him. Other examples are recorded, but the cancerous nature of the disease was not ascertained by the microscope. In all cases, however, these simultaneous tumours have existed in the *same* organ. The progress of primary encephaloid cancer is sometimes considerably delayed by its becoming surrounded by a kind of cyst, produced by the hypertrophy of the surrounding cellular tissue. M. Broca only admits the propagation of cancer by direct continuity, and is quite unaware of the grounds upon which Walshe assigns simple proximity as a mode of propagation. The cancers which become successively developed in the lymphatic glands are due to the transport of cancerous matter, which has obtained admission by the destruction of the walls of the lymphatics.

Proceeding from the local manifestations of cancer, M. Broca passes on to the consideration of the state of the economy in which *cancerous infection* manifests itself, and before examining into the nature of this, he details the changes in the fluids and solids by which it is characterized. We will pass over this portion as containing nothing novel. The author denies altogether the alleged antagonism of tubercle and cancer. Under the influence of cancerous infection, multiple cancers, termed by M. Broca *cancers of infection*, are produced in various organs, most of them being at more or less distance from the primary centre, and unconnected with it by direct communication. These act just as so many primary centres, propagating themselves around, and increasing the intensity of the infection to which they are due. Most of these cancers assume the encephaloid form.

M. Broca distinguishes the two very different conditions which precede and follow the appearance of the primary cancer, applying to the first

the appellation *cancerous diathesis*, and to the other, *cancerous infection.* The diathesis does not directly produce the infection, but it gives rise to the primary tumour, which at a later period poisons the economy. The tumour is, so to say, the bond of union between the diathesis and the general infection. Of what constitutes this modification of the economy which he terms the cancerous diathesis, M. Broca professes his complete ignorance; but he strongly opposes the hypothesis of Carswell, that the blood is primarily affected, as unfounded on any fact whatever. Even the relation which the primary tumour bears to the cancerous infection is at present obscure; but M. Broca considers the balance of evidence is in favour of the view which regards the infection as resulting from the transport of cancerous matter into the circulation, by means of the opened veins, and in exceptional cases, the lymphatics. Rejecting the metastatic theory, he contents himself at present with noting the facts connected with the appearance of the secondary or infection cancers, and acknowledges that he is as ignorant of the mechanism of their production as of that of the production of the pustules of variola, or the eruption of typhoid fever. M. Broca adds some illustrations to the melancholy chapter of *relapse* of *cancer;* which he regards as of nearly constant occurrence. Among the patients operated upon by Blandin in 1847 and 1848, there were 69 who furnished tumours, most of which, prior to the employment of the microscope, would have been regarded as cancerous. On examination, 2 proved to be fibro-colloid, 5 fibro-plastic, 15 epithelial, and 6 partial mammary hypertrophy. Of 39 really cancerous patients, 11 died from the consequences of the operation, and 28 survived these. Of these last, 19 were kept in view, and every one of them had relapse, 16 within the first year, 2 in the course of the second, and the last at the end of the twenty-fifth month. By the beginning of 1850, 17 were dead, and the other two were expected soon to follow them. In spite of so discouraging a result, M. Broca believes the *operation* should always be resorted to, whether for primary or relapsed cancer, providing general infection of the economy has not taken place. Sources of this infection are thus removed, and its production delayed. By those who confound the diathesis with the infection, the operation should, of course, be always interdicted.

John Chatto.

Review XIII.

On Rheumatism, Rheumatic Gout, and Sciatica: their Pathology, Symptoms, and Treatment. By Henry William Fuller, M.D. Cantab., Assistant-Physician to St. George's Hospital.—*London,* 1852. 8vo, pp. 403.

When we consider how large is the number of its victims, how intense and protracted the sufferings it occasions during its continuance, and what subsequent evils it entails from the cardiac mischief which on its subsidence it so often leaves behind, we cannot but feel convinced that the disease described as rheumatism is one of the most formidable affections to which the human body is liable. When we consider, too, that all this goes

on from month to month and year to year, at one season almost as much as at another, and apparently but little influenced by the epidemics and vicissitudes by which most of the other prevalent forms of disease are characterized, we can well understand why so many zealous inquirers have entered the field against this formidable enemy, with the view of discovering its nature, disarming it of its power, and reducing the number of its victims. And yet what can we really be said to know of the true pathology of this disease at the present time, after all the efforts which have been directed towards its elucidation. We certainly know that it occurs at all ages, but chiefly in the young; that it attacks the strong and robust scarcely less often than the weakly and debilitated; that it manifests itself by great febrile excitement attended with severe inflammation of a peculiar kind in the parts about the joints, and very often with serious cardiac disease; and that these phenomena are accompanied by profuse acid secretion from the skin, by the separation of large quantities of uric acid through the kidneys, and by a highly fibrinous condition of the blood. But as to the essential cause of these and other manifestations of the disease, we really know nothing with certainty, and the only approximation to truth we can be said to have attained, is, that these various phenomena are probably indicative of or dependent upon some, as yet undiscovered, morbid condition of the blood.

Among the more recent labourers in this wide and important field of investigation must now be ranked Dr. Fuller, the author of the work at the head of the present article. We purpose taking a general survey of the principal contents of this work, briefly alluding to such points as seem to present anything particularly novel or instructive, and entering somewhat more fully into one or two of the more important topics discussed in it.

An introductory chapter of twenty-eight pages is occupied with a consideration of the probable cause and nature of rheumatism. The author offers sufficient reasons against the opinion that mere exposure to cold and damp is of itself enough to induce an attack of the disease: yet, we think, he scarcely ascribes as much weight to the agency of this supposed cause as the history of many cases seems fairly to justify. For although it be true that the mere exposure to low temperature combined with moisture is not the essential, yet in a large majority of cases it seems clearly to be the exciting cause, acting on a system already predisposed to the disease. The real predisposing cause is ascribed by Dr. Fuller, in common with many other modern pathologists, to some morbid condition of the blood. In support of this view he observes, very justly, that—

"If certain substances are introduced into the circulation, fever is set up, rigors often occur, and inflammatory symptoms very shortly supervene in various parts of the body: symptoms which vary in intensity and locality, according to the amount and character of the poison. And if the blood be altered in character, it is practically the same, whether it contain matters foreign to the system, and altogether morbid in kind, or whether it contain an excess only of some material, a certain amount of which is compatible with health. In either case it is unhealthy and poisonous in its nature: in either case it contains a *materies morbi*, which may not only produce fever, or the symptoms of general derangement, but if irritating in its nature, may give rise to local inflammatory symptoms."

It is by no means clear, as yet, on what the morbid condition of the

blood in rheumatism really depends. Dr. Fuller believes it to be due to the existence of some poisonous principle probably "generated in the system as a product of mal-assimilation," and seeming to bear a "close identity with some natural excretion of the skin." Accordingly he entertains the opinion held by Dr. Prout, Dr. Todd, and others, that this poisonous principle, the actual *materies morbi*, is lactic acid, retained in the blood instead of being eliminated by the skin. No facts, however, are given in direct support of this view of the real nature of the morbid principle in the blood, and we must be considered as still in the dark respecting a question which bears so importantly on the pathology of rheumatism. All, however, will doubtless agree with Dr. Fuller, that whatever may be the morbid material or materials, on the existence of which in the blood the phenomena of rheumatism probably depend, they are generated within the system, and not absorbed from without; that they constitute a peculiar or specific poison, which is of the same general nature in every case; and, that the development of this poison may be called into play by anything which exercises a prolonged depressing influence upon the system, especially if at the same time there be an hereditary tendency to the disease. When the system is thus deranged, and the

"Rheumatic poison is present in it, any disturbing circumstance, even of temporary duration, such as over-fatigue, anxiety, grief, or anger, by rendering the system more susceptible of its influence, may prove the accidental or exciting cause of the disease, and exposure to cold or to atmospheric vicissitudes is almost certain to induce an attack."

Chapter II. is occupied chiefly with a consideration of the hereditary character of the disease, the author stating that he had distinctly traced the hereditary taint in 71 out of 246 cases of rheumatism admitted into St. George's Hospital: (nearly 29 per cent.) With regard to the influence of age and sex on the development of the disease, there is nothing strikingly new in the author's remarks. Most pathologists will concur in his observation, that

"Those persons are naturally the chief sufferers who, through want and privations, irregularity of life, and neglect of their general health, are rendered most liable to that state of mal-assimilation whereby the *materies morbi* is produced; and who, again, from the nature of their occupations, are most exposed to atmospheric vicissitudes, and to other exciting causes of the disease."

Chapter III. consists of a brief inquiry into the localities and textures of the body which are the seat of pain and inflammation in rheumatism. Although the fibrous and fibro-serous structures are those on which the stress of the disease mainly falls, yet since the disease is one of the blood, which circulates to all parts, so, the author thinks, "all must be more or less liable to suffer."

From a good general account of the symptoms of acute rheumatism in Chapter IV. we quote the following, as an important passage:

"One of the most remarkable and suggestive facts in regard to rheumatism is, that the fever and constitutional distress are not [always] commensurate with the extent and intensity of the local symptoms. Not only is rheumatic inflammation of the joints very frequently *preceded* by febrile disturbance, but sometimes the fever runs so high before any local symptoms have been established, as to cause even cautious and intelligent practitioners to mistake the nature of the impending

attack. Moreover, when febrile symptoms do thus precede the establishment of local inflammation, they are not only not increased by its occurrence, but, as was remarked by the sagacious and observant Sydenham, they are very generally relieved, the pulse becoming calmer, the countenance less anxious, and the patient altogether easier."

Chapter V., on the treatment of acute rheumatism, is one of the best in the book. After sketching in outline the several plans recommended by writers on the subject, and drawing the comparative merits of the different remedies usually employed, the author describes the course which he himself has been led to adopt. His observations on this subject are prefaced by remarks so appropriate in regard to the assumed efficacy of particular remedies in this disease, that we are induced to quote them. After remarking that the advocates of each remedy have founded their opinion of its efficacy on the rapidity with which the symptoms disappear after its employment, he continues, with justice,—

"But as, when uncomplicated by cardiac affection, the disease usually terminates, sooner or later, in recovery, and sometimes subsides with marvellous rapidity under every variety of remedy, it is obvious, that no sound inference can be drawn as to the success of any particular method of treatment, unless such treatment has been largely adopted, and has been attended with tolerably uniform results. And I am sure I may say, without fear of contradiction, that each and every plan of treatment which has been hitherto proposed is regarded by the profession as unsatisfactory. If in one person's hands any particular remedial course has proved efficient, it has signally failed in those of another: if at one time a remedy has proved efficacious, it has been found inert or injurious at another, under different circumstances of age, sex, constitution, and the like. Nor does this appear strange to those who consider the true nature of the disorder, and the variety of circumstances under which the physician may be called upon to minister to his patient's relief. The bleeding, which in the young, plethoric, and robust, may be necessary to allay excessive vascular action and cause free secretion, may, in the weakly, induce irritability of the heart, and a consequent attack of cardiac inflammation. The opium, which in one person may prove of the greatest service in promoting free perspiration, and in allaying the general irritability of the system, may, in another, check the biliary and other secretions, and thus prevent the elimination of the rheumatic poison. The continued use of calomel, and the constant purging, which may be beneficial to one patient, by removing large quantities of unhealthy secretions, may unnecessarily exhaust the strength of another, and tend very greatly to impede recovery. And so in regard to every remedy which has been proposed: what is useful at one time, proves useless or positively injurious at another."

The author then passes in review the principal measures hitherto recommended for the cure of rheumatism. Against the general abstraction of blood as a part of ordinary practice in this disease, he is strongly opposed, believing, with most physicians in this metropolis, that although venesection may be called for in some cases in which young robust persons are seized with a first and severe attack, yet even then it should be cautiously, and in other cases very rarely, employed, since even if the abstraction of blood does not tend to engender cardiac mischief, "still, convalescence is retarded, and the patient weakened and rendered liable to frequent relapses.' Although he speaks highly of the efficacy of freely clearing out the alimentary canal, especially as it is often loaded with dark unhealthy secretions, yet, he expresses "dissent from the practice of repeated active purging;" and this for three reasons:

"First, because it is not necessary to the cure of the patient, and, like bleeding, tends greatly to reduce his strength, and protract recovery. Secondly, because, from the nature of the complaint, the patient is quite incapable of moving, and his sufferings are aggravated, his irritability is increased, and his heart's action accelerated, by the repeated shifting of his position, which is rendered necessary by the calls of nature. And thirdly, because it necessarily gives rise to more or less exposure, which must be prejudicial to a person bathed in perspiration."

To keep up a free action of the bowels without undue purgation, he recommends calomel, with a full dose of opium, at night, and followed in the morning, if necessary, with a draught of rhubarb or senna with colchicum and potassio-tartrate of soda. The other main remedies in common employment are fully considered, including opium, mercury, antimony, colchicum, nitre, lemon-juice, and alkalies. But as there is nothing particularly novel in his views respecting the effects of these several remedies, we will pass over his remarks, merely observing that his experience does not enable him to speak highly either of nitrate of potash or lemon-juice, when employed alone; though of the value of alkalies and their salts generally, he speaks in the warmest terms. To this favourable opinion of the value of alkaline remedies, the profession generally is beginning to yield assent, especially as the opinion gains ground, that rheumatism is dependent on a morbid, probably acidulous, state of the blood; and that, in consequence, there is a preternatural tendency to the deposition of fibrine, which is counteracted by the free introduction of alkalies, by means of which the solubility of the fibrine is increased.

The general plan of treatment which Dr. Fuller advocates does not materially differ from that commonly practised, consisting principally of the free use of alkalies, combined with colchicum, calomel, and opium. Besides these internal measures, however, he employs, apparently with very marked advantage, the application of warm alkaline and opiate fomentations to alleviate the pain of articular inflammation. The solution he commonly uses for this purpose is composed of an ounce of carbonate of potash dissolved in a pint either of decoction of poppies, or of rose-water to which six drachms of Battley's solution is added; and he speaks most highly of its efficacy. "In every instance in which it has been employed, the relief obtained has been almost immediate, and the pain and inflammation have subsided rapidly."

The treatment of rheumatic disease of the heart is so closely mixed up with that of the articular inflammation, that they can scarcely be well considered apart. Therefore, although the author has dedicated a separate and distant chapter (chap. viii.) to this subject, we will take the liberty of appending to the remarks just made, the few observations suggested to us by the perusal of this chapter. Dr. Fuller strongly deprecates the free employment of mercury in acute rheumatism, preparatory to the onset of cardiac symptoms, and with the view of warding them off; for he observes, that when this remedy is given "so as to affect the constitution before the commencement of cardiac inflammation, it not only has no influence in preventing the disease, but by the irritability and general depression which it occasions, appears to modify its course in a manner by no means conducive to recovery." When the cardiac mischief, however, has fairly set in, he advocates, with others, the advantage of free mercurial action, for

the purpose of arresting the morbid process, and absorbing the materials deposited by it. Although, therefore, it may be fully conceded, that to push the influence of mercury to the verge of salivation previous to the development of cardiac symptoms would be objectionable, yet the cautious employment of this drug, especially in such cases where the occurrence of cardiac complication may justly be feared, with the view of gaining a certain hold upon the system, and so of saving time when the internal mischief fairly manifests itself, must, we think, be regarded as a salutary and judicious procedure. Combined with the use of mercury, the author advocates the employment of local depletion, effected by leeches rather than by cupping, as a less painful and distressing method of abstracting blood from the neighbourhood of the affected part. In other respects, the author's views on the mode of treatment of rheumatic inflammation of the heart, do not materially differ from those generally entertained.

Chapter VI. is dedicated to a consideration of the causes of rheumatic affection of the heart. The author makes it quite plain, that the cardiac affection cannot be due to a mere metastasis of the disease from the joints to the heart, but must be regarded as another mode of manifestation of the same morbid condition of the blood; for it not unfrequently happens that the heart is affected before the joints, while the disease sometimes appears to be limited almost entirely to this organ. He is of opinion that the occurrence of cardiac mischief is indicative of a severe form of the disease, rarely accompanying the sub-acute variety; and that it is most prone to arise in those cases in which the heart is either naturally irritable, or rendered so by previous active treatment or general debility.

In this chapter the author has almost necessarily had to notice that very important subject, the probable mode of origin of the fibrinous concretions, or warty vegetations, so frequently found adhering to the valves of the heart in fatal cases of rheumatic fever. Until recently, these warty growths have been almost universally ascribed to inflammatory exudation from the tissue of the affected valve; but latterly the opinion has been gaining ground, that they consist principally, if not exclusively, of fibrine deposited directly from the blood. This view, which is strongly maintained by Dr. Fuller, was brought very prominently before the profession, in 1850, by Mr. Simon, in his very excellent series of lectures on general pathology. Among the reasons which Mr. Simon there offers in support of this view are, first, the improbability of the lining membrane of the circulating apparatus becoming inflamed at all, since it contains no bloodvessels, and probably derives its nutriment from the stream of blood directly in contact with its inner surface. Yet, as observed by Dr. Ormerod, "this objection is more specious than real; for inflammation may be carried on by the same organic means as nutrition. And if, in any part, nutrition by unusual means effect an ordinary end, surely inflammation in the same part may do so likewise."* And it may be further urged, that even although the opinion, that the lining membrane of the circulating system depends, both for its ordinary nutrition and its morbid changes, on the blood in contact with its interior, and not on that in the vasa vasorum, be admitted to apply to the membrane lining the bloodvessels and general cavities of the heart, which seldom present evidences of disease, it can scarcely be supposed to hold good in

* Gulstonian Lectures, 1851.

the case of the valves; for both artificial injection and the vascularity induced by disease equally demonstrate the existence of a very free interstitial supply of blood to the textures both of the semilunar and auriculo-ventricular valves. And since it is on these parts that fibrinous deposits almost exclusively occur, and on which various unmistakable evidences of inflammatory action, such as general vascularity, swelling, and occasionally ulceration, are observed, we should be cautious ere we entirely discard the opinion, that fibrinous vegetations are, to some extent at least, dependent on inflammatory exudation, though probably increased by subsequent deposition from the blood. Against this inflammatory origin of these vegetations there are, however, other very important objections, which Mr. Simon, in common with others, strongly urges: for example, the far greater frequency with which the left valves in comparison with the right are affected, and the very striking and almost constant fact, that the fibrinous deposits are observed *only* on that side of the valve in contact with the moving blood. If inflammation were the cause of the warty growths, the right valves might naturally be expected to be as much affected as the left, since both are supplied by the coronary arteries; and both sides of an affected valve should present the exudations in equal amount, or if there was any difference, the side next the moving blood ought perhaps to show a smaller amount, owing to the liability of the stream to wash the deposit from the surface as rapidly as it is formed. By referring the fibrinous growths to direct deposition from the blood, both of these facts are explained; the left valves, being in contact with arterial and more highly fibrinized blood than the right, are more likely to become coated over with fibrine, while the surface directly opposed to the sanguineous stream is more likely to receive the deposit than the opposite one. The greater tendency of arterial than of venous blood to deposit fibrine when in motion, was repeatedly made the subject of direct experiment by Mr. Simon, who passed a thread through a main artery and contiguous main vein, leaving it for some hours to cut the stream: on removal, the portion of thread in the artery was invariably found coated with warty vegetations of fibrine similar to those on the valves of the heart, while that in the vein was never thus affected.

Against this weight of evidence in favour of the opinion, that the vegetations so frequently found on the valves of the heart in connexion with articular rheumatism are the result rather of direct deposition of fibrine from the blood than of inflammatory exudation from the tissue of the valve, must, however, be still opposed the fact already mentioned, that certain changes are sometimes observed, which can only be ascribed to an inflammatory process; as, for example, the soft, swollen, and vascular state of the tissue of the valve, but most especially to occasional ulceration. And since each of these, though sometimes observed to be unaccompanied by attendant fibrinous growths, are yet often found combined with them, the safest and perhaps the most correct view we ought at present to take of the vegetations in question is, that they may, in some cases at least, have a double source, being partly and primarily derived from inflammatory exudation, and subsequently enlarged by the deposition of fresh particles of fibrine from the blood flowing over the thus roughened surface, the tendency to such deposition being at the same time highly increased in consequence of the

blood being unduly charged with fibrine, and, perhaps from the existence of some acid within it, less able than usual to retain the fibrine in solution. Whether the opinion be correct, that in some, and if so in what proportion, of the cases, the fibrinous vegetations on the valves result exclusively from direct deposition of the fibrine of the blood, independently of any previous disease of the tissue of the valve, are questions which we are scarcely yet in a position to pronounce upon with certainty. Whatever view, however, he really held concerning the origin of the growths in question, there can be no doubt of the propriety of employing in the treatment of rheumatism those remedies which, such as the alkalies and neutral salts, especially those of potash, seem to possess the property of preventing the deposition of fibrine from the blood by increasing its solubility, and thus perhaps guard against any great increase of the vegetations, however they may have been formed in the first instance. Dr. Fuller briefly alludes to the possibility of the fibrinous deposits being occasionally detached from the valves; but he does not enter into the details which the admission of such a possibility almost necessarily entails. As this subject has been fully discussed by the writer of the present article, in the last volume of the 'Medico-Chirurgical Transactions,' it seems unnecessary to dwell upon it here.

Chapter VII. is a very important one, containing as it does an account of "Rheumatic Inflammation of the Heart, with its Pathological Effects, its Symptoms, Progress, and Terminations." The author treats, first, of inflammation of the pericardium, and after briefly alluding to the several products which may be effused from the surface of this inflamed membrane, and the mode in which they are subsequently disposed of, he says—"It has been suggested that the lymph effused in pericarditis is often thoroughly reabsorbed, so that the pericardium is restored to the condition of health, and perfect recovery takes place. This I do not believe to be the case."

Although thus doubting the possibility of complete removal of the effused lymph, yet in the next line he expresses the opinion, that "doubtless it may be reabsorbed to a very great extent:" one is therefore naturally tempted to ask—If absorption of lymph can go on to a certain extent, why, under favourable circumstances, should it not be supposed to continue until not merely a given quantity, but the whole of the effused lymph is taken up? Admitting the possibility of absorption at all, there is nothing to justify our assigning any limits to the extent to which it may go. Dr. Fuller does not enter at any length into the subject, adding little more than the expression of his opinion, that when a large quantity of lymph is poured out into the pericardium, "its complete absorption is next to impossible;" and that, therefore, as maintained also by others, "adhesion, more or less general, between the two layers of the pericardium is the most favourable issue we are justified in expecting, when pericarditis has been extensively diffused over the membrane." We think a more extended examination of this question would have been serviceable, especially since the opinion is now somewhat gaining ground, that general inflammation of the pericardium does not terminate in adhesion so frequently as it was thought to do, and that such a termination is not the most "favourable" one that nature could accomplish under the circumstances. In a paper on this subject, which the writer of the present notice published about three years ago,* he endeavoured to show

* Medical Gazette, 1850.

that pericardial adhesion was a rare event in comparison with the frequency of pericarditis; and subsequent observation has tended to strengthen the opinion then expressed. Many difficulties lie in the way of demonstrating the correctness of this opinion; and although much may be done by mere reasoning, yet it is only by repeated and careful observation that its truth can be established. Cases, however, have been observed in which death has occurred at a comparatively remote period after a pericardial friction-sound has been heard, and yet no adhesion of the pericardium has been found. This seems to prove that adhesion is not an invariable result of the effusion of inflammatory lymph into the pericardial sac. The favourableness of such a termination is rendered questionable by the cardiac symptoms noticed during life, and the enlargement of the heart found after death in some of the cases of adherent pericardium; while the absence of these effects in other cases cannot justly be considered to prove that the heart tolerates without resentment this crippling and hampering of its free movement; for in the majority of these latter cases, there may usually be found enough to explain the absence of evident signs of cardiac disease.

It may, indeed, be stated, as a general rule, to which we think there will be found but few, if any, real exceptions—that permanent adhesion of the pericardium, whether universal, or involving only a comparatively limited portion of the serous membrane, will, by impeding and embarrassing the action of the heart—which it cannot, in reason, be supposed not to do—naturally tend to induce the same hypertrophied condition of the muscular tissue of this organ that any other impediment to its free action, whether resulting from valvular imperfection, arterial disease, or such a morbid condition of the blood as exists in Bright's disease, is universally admitted to be capable of producing. In any one of the cases, the amount of hypertrophy observed may be determined in some measure by the comparative demand made upon the action of the heart. If, from favourable circumstances, this organ be kept comparatively quiet, the degree of hypertrophy induced may reasonably be expected to be less than when this tranquil state has not been maintained. So, too, the amount of hypertrophy discovered after death, may naturally be supposed to bear some relation to the state of general nutrition of the muscular and other tissues in the rest of the body: for if the body generally is wasted and emaciated, either from simple inanition, or from any disease materially interfering with nutrition, such as cancer of the stomach or tubercular ulceration of the intestines, or the like, one cannot feel surprised if the heart, in common with other parts, should give evidence of imperfect nutrition of its muscular walls, especially when it is remembered that the enfeebled state of the body existing in such cases would furnish the least favourable circumstances for the development of hypertrophy, notwithstanding the existence of a formidable mechanical obstruction to the action of the heart. Therefore, it is scarcely consistent with common reasoning to conclude, that because in a given case of adherent pericardium the heart is not enlarged, therefore pericardial adhesion does not tend to produce impediment to the heart's action, and consequent hypertrophy of its walls to obviate and counteract this impediment: for, to make the conclusion at all exact, it ought to be shown in such case, that the patient had not lived under conditions of rest and comfort, in which all undue action of the heart had

been scrupulously guarded against, or had not died under circumstances attended with general wasting of the muscular and other soft tissues of the body. And we venture to express the firm belief, that where conditions such as these have not existed, the heart which is trammelled with an adherent pericardium will, in almost every instance, be found associated with enlargement of the heart, and this, whether there be valvular disease or not.

In regard to this part of our subject, we have re-perused with much interest a paper recently written by Dr. W. T. Gairdner,* on what he terms the "favourable termination of pericarditis." Although the first and principal conclusion at which the author arrives is to the effect, that "a *considerable* majority of the cases of adherent pericardium is unattended by enlargement of the heart," yet a careful examination of the ten cases on which this conclusion is principally founded, convinces us that his fourth inference or conclusion, "that, in some circumstances at least, adhesion of the pericardium, originally uncomplicated, may lead to very great hypertrophy and dilatation of the heart," more nearly expresses the real truth than the first. For, by his own showing, all the ten cases in which an adherent pericardium was found unaccompanied by enlargement of the heart, proved fatal under conditions of more or less extreme general wasting and cachexia, which, according to the opinion we have just stated, would serve to explain the absence of the hypertrophy.

The remainder of this chapter is occupied with a lengthened account of the appearances presented, and subsequently undergone, by the fibrinous vegetations on the valves of the heart; and of the general and physical signs of rheumatic pericarditis and endocarditis, in which, however, we do not notice anything particularly new.

In Chapter IX. there are some useful statistics of "heart-disease in connexion with rheumatism," deduced principally from the analyses of 379 mixed cases of acute and subacute rheumatism treated in St. George's Hospital. Among the general results of this analysis, we find that the author agrees with the observations of Dr. Latham, M. Bouillaud, and others, that some cardiac affection arises in about one-half of all cases of *acute* rheumatism; while of *subacute* cases, as noticed by the author, not more than about one-third are attacked with cardiac mischief. From an analysis of the recorded experience of Dr. Latham, Dr. Taylor, Dr. Macleod, and others, as well as from the author's own observations, he deduces that the proportion of cases in which pericarditis ensues in the course of acute rheumatism is as 1 in every 5·97; the proportion of endocarditis (or at least of endocardial murmur) is 1 in 2·25; while the proportion which recent pericardial bears to recent endocardial affection is as 1 to 2·9. In many of the cases of supposed endocarditis, however, he believes that the murmur on which the supposition of inflammation is founded, results either from purely functional causes, or from "temporary imperfect closure of the mitral orifice, consequent on irregular contraction of the structures connected with the valves, or by the presence of fibrine deposited on the valves, without the concurrence of endocardial inflammation;" and he is of opinion that

"Somewhat less than one-third of all recent cardiac murmurs met with among patients suffering from acute rheumatism, will be found to result from pericarditis,

* Edinburgh Monthly Journal, 1851.

and somewhat more than one-third from endocarditis, while the remainder will be referable to one of the three causes above specified, as contributing to the production of valvular murmur."

In Chapter X., which is occupied with "Affection of the Brain, Inflammation of the Lungs and Pleuræ, and Disorganization of the Joints," occasionally attendant upon rheumatism, the author develops in full the very probable opinion, that the singular cerebral and spinal symptoms which sometimes arise in this disease, are dependent, not upon organic affection of the nervous centres, but upon a morbid condition of the circulating blood producing irritation in the brain and spinal cord, previously disordered, or rendered unduly susceptible to morbid impressions.

The remaining three chapters of the book are devoted to Rheumatic Gout, Chronic Rheumatism, and Sciatica and other forms of Neuralgic Rheumatism; but into these we do not purpose entering.

Having given an analysis of some of the principal contents of this work, it only remains to state the impression which a careful perusal of its pages has left upon our mind—namely, that although there is nothing strikingly novel in the manner in which the author has dealt with his subject, or in the opinions set forward by him, yet the volume contains a large amount of valuable and instructive information, clearly and connectedly put together; and while it may be said to represent very fairly the extent and kind of our present knowledge concerning the symptoms and pathological nature of the important disease of which it treats, it furnishes us with sound and judicious views in regard to the most appropriate mode of dealing both with the rheumatism itself and the complications so apt to arise in its progress.

W. Senhouse Kirkes.

Review XIV.

Des Epidémies :—Thèse soutenue au Concours pour une Chaire d'Hygiène, Févr. 23, 1852. Par M. Marchal (de Calvi).

On Epidemics. By M. Marchal (de Calvi).—*Paris,* 1852. 4to, pp. 235.

There is probably no department of medicine which occupies a larger share of attention at the present time, with the general public as well as with the profession, than that which relates to the spread of epidemic diseases: and there is no inquiry which seems to us more likely to yield results of the highest and most extensive advantage. It is on this account, that although we are opposed on principle to the multiplication of associations for special objects, we have welcomed the establishment of the Epidemiological Society; as likely to direct into one channel a large amount of information, which is at present dispersed over a wide range, and to bring to bear upon it an intelligent scrutiny that shall select the points most worthy of consideration, and a power of philosophical combination that may draw forth some general principles, the attainment of which shall give a positive direction to further investigation, as well as afford a comprehensive expression of the facts already determined. We feel convinced that this is a subject on which mere *empirical* generalization will do very little. A vast mass of facts may be collected and tabulated—

the numerical method may be applied to these in every conceivable mode —and yet, so long as no *connecting idea* is discerned among them, they remain utterly incapable of serving for the establishment of those general principles, which enable true Science to predict with certainty what *will* or what *will not* occur in any given contingency, and which therefore furnish the only satisfactory basis for the rules of Art as to what *should* or *should not* be done. "Un fait qui n'emporte pas une idée, duquel on ne peut s'élever pour voir plus loin," says M. Marchal (quoting from Montesquieu), "est un caillou qui ne vaut pas la peine d'être ramassé, et qu'il faut, au contraire, repousser du pied pour en débarrasser la route!"

It is in this spirit that M. Marchal professes to have investigated the subject of epidemics. He has been desirous, he tells us in his preface, of establishing a doctrine, and of bringing all the elements of the discussion into convergence upon one single idea, which he believes to be novel, and on which he thinks that the practical rules of prophylaxis may be most securely based. From such a preliminary flourish of trumpets, we naturally expected something of greater value than the conclusions at which our author has arrived; which are nothing more than that plague, yellow-fever, cholera, and intermittent fevers of all types, are diseases of paludal or malarious origin; that typhus and epidemic typhoid fevers, dysentery, &c., are engendered by animal miasmata; and that mixed forms of disease may be produced by the combined action of these different influences. Our readers will recollect that a doctrine of this kind was long since put forth, and sustained with great ingenuity, in our own pages (vol. iii. p. 74, et seq.).

It is not our present purpose, however, to discuss the *origin* of the poisons, whose introduction into the system gives rise to the various forms of epidemic disease. We shall not enter upon the question of the relation of these diseases to each other, nor inquire why, in the very same localities, and under the identical circumstances, so far as can be traced, different forms of epidemic disease prevail at successive periods, typhoid fever giving place to typhus, typhus to cholera, cholera to scarlatina, and so on. The complete extermination of such diseases, by the annihilation of the causes from which they originally spring, may or may not be within the reach of medicine; but it will need a far larger amount of evidence than we at present possess, or are likely soon to attain, to enable us to determine with anything like certainty what these causes are, and what is man's power over them. There is, however, a far more ready method of greatly mitigating the severity of these epidemic visitations, if not of absolutely keeping them at bay; and this is, by bringing the mass of our population under conditions which shall destroy their liability to become the subjects of such diseases. This subject has been several times dwelt on in our pages; but it is one of such vast importance, that it can scarcely be pressed too often upon the attention both of the medical and general public; and we revert to it now, because we believe that we can bring forward a connecting idea, which is a legitimate generalization of a vast assemblage of facts, and which, if admitted, will be found very fruitful both in practical and in scientific applications.

In every Etiological inquiry, it behoves us to set out with a definite idea of the meaning of the term *cause;* and to a vague and imperfect

apprehension of the legitimate sense of this term, we trace much of the unsatisfactory character which appertains to medical inquiries generally. The true object and bearing of our inquiry is altogether lost sight of, in fact, unless we regard causation as consisting in *unconditional sequence*—that is, unless we recognise the *concurrence of a certain set of conditions as being essential to the certainty of the result, which invariably occurs when those conditions are fulfilled, and never occurs save when they are in operation.* The doctrine of causation in medicine cannot be different from that which holds good in every other science. It is only in appearance that the causes of disease, the action of remedies, &c., are uncertain. If we knew *all* the antecedent conditions that are present in each case, we should, doubtless, find the consequents as invariable as they are elsewhere. And it is only by the recognition of this principle, that we are led to the search for those more recondite conditions, which determine a marked difference of result, when, from the similarity of those which are more ostensible, we have been led to expect a repetition of that which we have before observed. It is a great mistake, as Mr. Mill has most clearly demonstrated,* to single out any one antecedent, and designate *it* as *the cause*, assigning to the rest a subordinate place as mere *conditions*. All are alike necessary to the result, if this invariably follows them when they are all present, and is no less invariably wanting when any one of them is withdrawn. Considered under this point of view, the "predisposing causes" are just as essential to the production of the resultant disease, as are those "exciting causes" on which our attention is usually more strongly fixed. The cholera-poison conveyed to our shores, whatever may be its mode of transport, owes its potentiality to the condition in which it finds the subjects of its invasion. If an average hundred of the inhabitants of our island were to receive the most powerful dose of cholera-poison that has ever been concentrated within it, probably not more than one in ten would be affected by the disease in its full intensity; the remaining nine-tenths either escaping altogether, or suffering only from diarrhœa of greater or less severity. In the ordinary phraseology of medicine, the individuals who suffer most are said to be *predisposed* to the disease; and collective observation has enabled the pathologist to specify, with considerable certainty, those antecedent circumstances, which, when present, act as predisposing causes, and the absence of which, on the other hand, effectually prevents the production of the disease.

It is remarkable, however, that *any one* of these predisposing causes, joined to the exciting cause (i. e. the presence of the cholera-poison), seems able to determine the result. Putrescent food, foul water, offensive effluvia, imperfect ventilation, &c., may operate singly and separately; or they may act in combination; and their separate action, if sufficiently intense, seems fully as capable as their combination in lower degrees, of giving potency to the specific poison. Hence it would at first sight appear difficult to apply to this subject the doctrine of invariable and unconditional antecedence; finding, as we do, that so many antecedents, apparently differing greatly in character, may each produce the result, when acting concurrently with one other condition. But if we look a little deeper than the surface, the question is naturally suggested to our minds, whether the really inva-

* Elements of Logic, book iii. chapter v.

riable antecedent be not *some condition of the human body*, which they all in common tend to produce, and which is that which the cholera-poison requires for its potential action. It is obvious that if any such condition can be distinctly specified, we come to possess a more satisfactory *rationale* of the *modus operandi* of the known predisposing causes of epidemic diseases, than we have yet obtained; we shall be able to predict their operation with greater certainty in new combinations; and we shall be guided in our search for others which are as yet hidden from our observation.

But further,—experience has shown that what is true of cholera, is true also of a large number of other diseases of the same class; the causes which engender a special liability to one, being equally efficacious in producing a similar liability to another: so that we have a strong presumption that the *cause* of the invasion of epidemic disease in each individual case, may be expressed by this simple formula,—a certain general condition of the human body, *plus* the specific poison of the particular disease. When these two antecedents are concurrently present, the disease is invariably produced; when either of them is wanting, the disease cannot be developed. And we may thus prevent its invasion no less effectually by preventing the occurrence of the predisposition, than by preventing the development of the poison itself. Now as the conditions of the former are for the most part both certainly known and readily within our reach, our first attention ought surely to be given to them; the other question, however, being not disregarded, although less capable of an immediate solution. Towards this solution, however, we shall have made an important step, if we can clearly determine that condition of the system which is essential to the operation of the morbid poisons; since we thereby gain a clue to the nature of that operation, which may be of great importance in leading us to a precise knowledge of it.

Now we believe that we shall be able to show, that all the recognised predisposing causes of epidemic disease, tend to produce, in the blood of the individuals exposed to them, an excess of those decomposing organic compounds, which, as physiology teaches us, are always present in the circulating current, in minute proportion; being conveyed by it, from the spots in which they are introduced, or in which they are generated, to the organs through which they are to be eliminated. Such an excess is manifestly producible, either by the direct introduction of these matters from without, in the food or drink consumed, or in the air respired; or by the production of them within the body, at a rate beyond that at which they are normally eliminated; or by some obstacle to their elimination, which prevents the amount ordinarily generated from escaping at its normal rate through the usual outlets. And as we shall find that to these three categories all such causes may be referred, we have very strong ground for the belief, that the condition of the blood to which we have alluded, is that which, in concurrence with the specific poison, affords the invariable antecedent of which we are in search.

Assuming, for the moment, the truth of this conclusion,—which we think we shall be able to substantiate to the satisfaction of our readers, by a subsequent examination of the *modus operandi* of each of the predisposing causes upon the living body,—let us stop to notice its bearing upon our notion of the *modus operandi* of the specific poison. All the epidemic

diseases with which we are acquainted, belong to that class, which the term *Zymotic* (first suggested by Mr. W. Farr, in the Reports of the Registrar-General) has now come, by a sort of general consent, to be applied in this country. By some, it is true, this term has only been employed as a convenient designation for a group of diseases presenting a strong family resemblance one to another. But those who use the designation in the sense in which it was originally proposed, mean by it to imply, that the specific poisons of these diseases act on the blood in the manner of *ferments;* a doctrine which, vaguely recognised in our older pathology, has acquired precision within a comparatively recent period, from the researches of Liebig and others on the essential nature of ferments, and on the variety of modes in which they may operate in the animal body. A *ferment*, it is now clearly understood, is an azotized substance in a state of putrefactive alteration; the changes which are taking place among its components having the power of exciting changes of analogous character in certain other substances brought into contact with it, whereby new compounds are formed, into the composition of which the elements of the ferment do not enter. The influence of the ferment may be regarded, therefore, as not *material* but *dynamical;* consisting in the *propagation of force*, not in the introduction or substitution of components; and hence it is that ferments derive their extraordinary potency, extremely minute quantities of them being capable of effecting most important and extensive changes in fermentible substances brought within the sphere of their activity. The nature of the product depends upon (1) the nature of the ferment, (2) the nature of the fermentible substance, and (3) the stage of decomposition of the ferment; very minute differences in any one of these particulars being competent to determine most important variations in the result.

The substances on which ferments have a special action, may be ranged under two heads, the azotized, and the non-azotized. In the former, they tend to induce a change which enables them, in their turn, to act as ferments; whilst in the latter, they simply occasion that re-arrangement of their components in which fermentation consists, these substances having no power, even whilst themselves in the act of change, of inducing a similar change in others. Hence the potency of a ferment will depend, not merely upon the activity of the change which it is itself undergoing; but also upon the presence of some azotized substance in which it can excite the same change, and, through which it can act upon other fermentible matter. Thus if a small quantity of yeast be introduced into a solution of pure sugar, the amount of fermentation which it will induce is very limited, and no reproduction of the ferment takes place; for the dynamical action of the yeast does not extend itself to the whole mass of the sugar, and completely ceases as soon as the yeast has itself gone through its entire transformation. But, on the other hand, if the yeast be introduced into a solution of malt, which contains not only sugar, but the albuminous constituent of the corn-grain, its main action is upon the latter, exciting in it an analogous change, whereby not only the whole sugar is made to undergo fermentation, but a reproduction of the ferment is effected, to many times the amount originally introduced.

Now this fact has been already applied by the advocates of the zymotic hypothesis, to the exanthemata and other zymotic diseases (such as hooping-

cough) which usually occur only once in life. It has been supposed that the specific poisons of these diseases cannot act upon pure healthy blood, but that the presence of some special fermentible element is necessary to enable them to be developed within the system; and that when this has been changed by the agency of the ferment, and has been eliminated from the blood, that particular form of zymotic poison can no longer take effect. In this case it is further necessary to suppose, that the renewal of the liability, which occasionally manifests itself in a second attack of some one of these diseases, is consequent upon a reproduction of the fermentible matter. By some, this hypothesis has been rejected as altogether improbable, and as being entirely incapable of proof. By others, it has been provisionally adopted; the argument from analogy being considered sufficiently valid to justify its acceptance as a *possible* explanation of the facts of the case, and therefore as entitling it to hold its ground until displaced by a better one. We think that the cumulative evidence we shall adduce respecting the conditions under which zymotic diseases in general tend to develop themselves, adds great weight to this hypothesis, and affords to the zymotic theory generally a confirmation that seems, whilst stopping short of actual demonstration, adequate to substantiate its essential truth; since it is scarcely possible that the large number of coincidences which we shall presently point out, can be accounted for on any more satisfactory principle.

Our general proposition, then, when stated (so to speak) in the terms of the zymotic theory, amounts to this:—that all the recognised predisposing causes of zymotic disease tend to produce in the blood an undue accumulation of azotized matter, already in a state of *retrograde metamorphosis*, and therefore precisely in the condition in which it is most readily acted on by ferments; that the presence of such matter is absolutely requisite, in the great majority of cases, for the morbific action of the zymotic poison, which has no direct action upon healthy blood, all whose components are in a state of progressive metamorphosis; and that the liability of each individual, among a number who may be concurrently exposed to the same poison, will mainly depend upon the degree in which his blood may be charged with the matters in question *

I. The first indication which we shall adduce, of the actual existence of this hypothetical entity, is that which first suggested the doctrine to our own minds; namely, that peculiar liability of the *puerperal* female to become the subject of zymotic disease, which affords the plainest evidence (if the zymotic theory have any truth whatever) of her peculiar susceptibility not merely for the reception, but for the further development, of poisons

* There are certain zymotic poisons, whose action is so powerful as to be able to exert itself on blood in a perfectly healthy state; such, for example, as the venom of serpents, the pus of pustule maligne, the sanies of glanders, the cadaveric matter of the subjects of particular diseases, the syphilitic poison, &c. But with regard to all save the first of these, there appears strong reason to believe that the potency of their effects is, in great measure, dependent on the previous condition of the system; the worst results being generally seen in those who have been subjected to the influence of some one or more of the predisposing causes to be presently enumerated. Thus, it is notorious that the dissecting-room poison is more virulent in its effects towards the *end* of the winter session, when the students exposed to it have been living for some months amidst putrescent effluvia, as well as (too frequently, at least,) in an atmosphere otherwise vitiated by want of ventilation, and have been impairing their health by over-study or by other irregularities which tend to impart to the degenerative processes an undue excess; than it is when the same set of individuals come fresh from the country, with their blood purified by active exercise and exposure to fresh air, and with their systems invigorated by wholesome refreshment and mental repose.

of the zymotic kind. Upon this point we apprehend that not the slightest question can exist, in the minds of any who have attended to the accumulation of evidence which now exists, with regard to the communicability of the puerperal infection, and with respect to the production of the various forms of puerperal fever by the introduction of some other *materies morbi* than that which might be considered peculiar to it. Thus, it is well known to be a fact of no unfrequent occurrence, that if a medical practitioner or a nurse go from a case of puerperal fever to attend other puerperal females, the chances are that these, too, will be attacked with the disease, although no epidemic of puerperal fever may exist at the time; and not only so, but even if such communicating individuals should abandon obstetric practice for weeks or even months, there is a great risk of the occurrence of this fearful disease in the very first puerperal patient with whom they may come into intimate relation. But further, if the antecedents to the *first* case of puerperal fever be strictly inquired into, it will very probably be found that the practitioner or nurse had come to the patient from attendance on a case of scarlatina, typhus, erysipelas, or small-pox; there being strong evidence that the specific poisons of the first three of these diseases, if not of the last, develop themselves in the form of puerperal fever, if introduced into the system of a parturient female. We cannot refer to a more "pregnant instance" of the peculiar receptivity of her system for zymotic poisons of even a non-specific description, than that which has been afforded by the experience of the Vienna Lying-In Hospital; in which, as our readers will remember,* a mortality of from 400 to 500, in an average of 3000 deliveries per annum, appears clearly traceable to the introduction of cadaveric matters, through the uncleanliness of the attending students; these matters being especially potent, when derived from the bodies of those who have died from the adynamic forms of zymotic disease. We learn from a communication made to Professor Murphy by Dr. Routh,† that the occurrence of puerperal fever in one of the Parisian lying-in hospitals, is traceable with equal certainty to the putrescent effluvia of a neighbouring abattoir; for, whilst the wind blows in such a direction as to waft these effluvia *from* the hospital, there is no disposition to puerperal fever; but the reversal of its direction, and a diffusion of the putrescent emanations through the hospital, is followed by an outbreak of that disease. There can be no doubt whatever, that a foul state of the atmosphere of the lying-in room, consequent upon deficiency in its ventilation, greatly favours the operation of the poison, if it be not actually capable of generating it; and we believe that there is adequate evidence, that the poison may be actually produced by the putrescent miasmata arising from deficient sewerage of a lying-in hospital.

We need not dwell upon the destructive ravages of this terrible disease, so fatal to those whom it attacks, and so pertinacious in its preference for the wards of a particular hospital, or the patients of a particular accoucheur. Our business is to inquire, what there is in the constitution of the puerperal female, which gives to the poison its special potency; and the answer to this question we find in one of the causes recently assigned by Dr. West‡

* British and Foreign Medico-Chirurgical Review, vol. v. pp. 273, 468.
† Medical Gazette, June 28, 1850, p. 1113.
‡ Medico-Chirurgical Transactions, vol. xxxiv. p. 75.

for the ill success of the Cæsarean section;—namely, the fact that the uterine tissue must be in a state of rapid disintegration; the evidence of which has been obtained by the observations of Virchow, Kilian, and Kölliker, as to the state of fatty degeneration of its muscular substance, and the presence of abundant fat globules in the lochial discharge; and (we may add) by the observations of Dr. Retzius (brother of the distinguished professor of anatomy at Stockholm) upon the abundance of fat globules in the urine of the puerperal female. We look upon this degeneration as only a peculiar case of that which is constantly taking place in muscular tissue as a consequence of its functional activity; for although we may not yet be in a condition to specify the precise nature of the chemical changes which occur in it, there is adequate evidence that these changes involve the ultimate production of two classes of *complementary* compounds; on the one hand, a *non-azotized* series, including sugar, fat, and lactic acid, which are destined to be eliminated by the respiratory process; and on the other, a *highly-azotized* series, of which creatine and urea are among those that are first traceable, and to which the kidney affords the appropriate channel of exit. After the mighty effort which the uterus has made in the expulsion of its contents, rapid disintegration of its substance occurs: and there can be no doubt that, as the *complement* of the fatty compounds which are generated more abundantly than they can be carried off by the respiratory process, there must also be produced an equivalent amount of highly-azotized compounds; such as, until they have assumed the comparatively permanent forms of urea and creatine, must be, like the azotized matter of sweet-wort, peculiarly susceptible of having their constitution entirely altered by the action of a ferment.

II. Now if this be true of the puerperal state, it ought to be true also of any other condition of the system, in which a rapid disintegration of tissue is going on; and we are naturally led to inquire, whether any peculiar receptivity for poisons of the kind we have alluded to, exists in those who are suffering under severe injuries, and whether in such individuals they develop themselves in a form of peculiar malignancy. We need not, we think, enter into any laboured argument to prove, that such a class of phenomena *is* presented by the peculiar liability of this class of subjects to a certain form of adynamic fever, which has hence received the special designation of "Surgical Fever," as well as to erysipelas. And our view of the case is most strikingly confirmed by the results of the inquiries of Professor Simpson; who has been led to conclude (1) that Surgical Fever is as much communicable from patient to patient by the hand of the surgeon, as Puerperal Fever is by that of the accoucheur; and (2) that a reciprocal relation exists between these two diseases, each being able to generate the other, so that they are to be regarded as the manifestations of one and the same *materies morbi*, their differences being dependent upon the peculiarities in the condition of the two classes of subjects.*

III. From the consideration of the cases in which the predisposition to

* See Professor Simpson's essay "On the Analogy between Puerperal and Surgical Fevers," in the Edinburgh Monthly Journal, vol. xi. p. 414; and the communications of Professor Simpson and Dr. Moir, to the Medico-Chirurgical Society of Edinburgh, in the Edinburgh Monthly Journal, vol. xiii. p 72.

zymotic disease appears to be given by the existence of some peculiar source of degenerating, and therefore fermentible matter, *within* the living body, we pass on to those in which matter of this kind can be shown to have been introduced *ab extra.* Here, again, the evidence which experience affords in support of our position, is both cogent and abundant; for all who have watched the progress of epidemic diseases, are well aware that the habitual or even occasional ingestion of putrescent *food*, or of *water* charged with decomposing matter, or the inhalation of *air* loaded with miasmatic emanations, are among the most powerful of those "predisposing causes," by which the attack is determined in any individual case. Particular examples of this kind were abundantly furnished during the last visitation of cholera; and some of them are chronicled in the Report of the General Board of Health (pp. 63, 64). One instance, however, which has not yet found its way into print, we may especially notice. Our readers may recollect that just at the time when the decline of the epidemic in the Metropolis, and generally throughout the country, occasioned a "Thanksgiving Day" to be ordered by our Government, there was a remarkable and (so far as it extended) a very fatal outbreak of cholera at Bridgwater and Taunton. Whether the zymotic agency was or was not present in these two places in any peculiar intensity, we have scarcely sufficient means of determining; but this much is certain, that a considerable part of the severity of the attack was clearly attributable to the condition of the subjects of it; this condition having been induced, in a portion of them, by the ingestion of decomposing food, and. in another portion, by the respiration of an atmosphere deteriorated by over-crowding. The circumstances of the Bridgwater outbreak, as related to us by Dr. Brittan (who was sent down by the Board of Health to inquire into them), are as follows:—A cargo of oysters, spoiled by long detention in the Bristol Channel, had been brought to the town; and the sale of them having been prohibited by the authorities, on account of their putrescent condition, they were given away to any who would receive them; and several of the children in a neighbouring school partook of them plentifully. In the course of the following night, *all* the children (so far as Dr. Brittan could ascertain) who had partaken of the oysters, were attacked with cholera and choleraic diarrhœa; and eleven of them died the next day. Now, had the same occurrence happened, when no special zymotic poison was present, we apprehend that the mischievous effects of this unfortunate feast would have been confined to a simple diarrhœa, by which the noxious matter would have been carried out of the system. But a zymotic poison being present, this found in the decomposing matter the condition most favourable to its development; and it therefore fastened itself on the systems of those, in whose blood such matter was circulating.

The special cause which determined the severity of the Taunton outbreak, will come to be considered under another head; and we shall now only advert to another case, in which a peculiar liability to the attacks of zymotic disease seems to be engendered in an entire population, in consequence of their habitual ingestion of putrescent food. Referring our readers to our seventh volume (p. 419), for an analysis of Dr. Panum's 'Observations on an Epidemic of Measles in the Färoe Islands,' we may remind them that these islanders live during a large part of the year upon

meat in a state of incipient decomposition; and acquire such a liking for the highest possible goût, that *rast*, or half decayed maggotty flesh, fowl, or fish, is commonly introduced as a special relish at the end of a meal. The result of such a diet (as might be anticipated) is a continual disorder of the digestive organs, manifesting itself especially by diarrhœa; and this is a symptom of annual occurrence on the bird islands, and is also invariably observed after a large "take" of whales, when much of the flesh of these animals necessarily becomes *rast* before it is consumed. Now this diarrhœa complicates the course of other diseases, and may even become, from its obstinacy and exhausting character, their most serious occurrence; as was particularly observed by Dr. Panum, in regard to the epidemic of measles, which he specially investigated. Notwithstanding this, the ordinary rate of mortality among these people is very low (only 1 in 64$\frac{3}{8}$ annually), and a large proportion of the population live to a good old age; a circumstance which we attribute to their active, hardy lives, and their habitual exposure to a low external temperature; since both of these causes will tend to favour the elimination of the noxious matter as fast as it may be introduced, by promoting the activity of the respiration, which causes its speedy oxidation. If a population subsisting on the same diet, were transferred from the open air of the Färoe islands, to close ill-ventilated dwellings in some tropical country, we cannot doubt that the evil would be most fearfully aggravated; and even in the Färoe islands, although the general rate of mortality is so low, yet there is such an extraordinary liability to the spread of epidemic diseases, that when they *are* introduced (which is fortunately at rare intervals) they spread like wild-fire through the entire population. Thus, the epidemic of measles investigated by Dr. Panum, attacked in the course of six months scarcely less than 6000 out of a population of 7782; no age being spared, and very few individuals escaping, save such as had suffered from the malady in the epidemic which had occurred sixty-five years previously, and such as maintained a very rigorous isolation.

IV. That the habitual ingestion of decomposing matters in the water used as drink, is capable of inducing a similar liability to zymotic diseases, scarcely admits of a question. We extract the following from the Cholera Report of the Board of Health (p. 62):

"In Manchester a sudden and violent outbreak of cholera took place in Hope-street, Salford. The inhabitants used water from a particular pump-well. This well had been repaired, and a sewer which passes within nine inches of the edge of it became accidentally stopped up, and leaked into the well. The inhabitants of 30 houses used the water from this well; among them there occurred 19 cases of diarrhœa, 26 cases of cholera, and 25 deaths. The inhabitants of 60 houses, in the same immediate neighbourhood, used other water; among these there occurred 11 cases of diarrhœa, but not a single case of cholera, nor one death."

In this instance, it seems impossible to avoid the conclusion, that the impregnation of the system with the putrescent matter thus insidiously introduced, determined not merely the selection of individuals by the cholera poison, but the terrible fatality of the disease in those attacked by it, *only one out of twenty-six having escaped.*—We could cite, from the same Report and its Appendices, many other cogent examples of the same kind, several of

them extending the evidence to zymotic diseases of different forms; but such of our readers as may not be satisfied upon this point, can easily refer to these sources of information; and we shall confine ourselves to the mention of a case, which occurred within our own knowledge. In a certain terrace, in the most aristocratic suburb of a large provincial town, consisting of houses of a superior class, and very favourably situated as regards free access of pure air, an epidemic of gastric fever broke out, a few years ago, much to the astonishment and dismay of the residents, no such malady having been known to prevail in the neighbourhood within the memory of "the oldest inhabitant." It was soon observed, however, that this epidemic was limited to particular houses; and, further, that whilst in some houses whole families were more or less affected by it, in others only the servants were attacked. Now the water-supply of this terrace was in part derived from a neighbouring well, and in part from a deep spring at a distance; the latter, however, being paid for, was by no means universally employed, although much preferred as a purer water; and thus, whilst some of the houses only used the spring-water, others only used the well-water, and in others, again, the spring-water was employed in the parlour, the well-water by the servants in the kitchen. For some little time before this outbreak of fever, a disagreeable taste had been observed in the well-water; and this was subsequently traced to the bursting of a sewer, which had discharged part of its contents into the well. The houses *first* attacked by the fever, were those in which only the well-water was employed; and it was where the use of well-water was limited to the servants of the family, that they alone were at first affected by it. There appeared reason to think that some of those who had not used the well-water became the subjects of the malady by subsequent communication with those first affected; but there seems no reasonable doubt, that the contamination of the water was the cause which originally determined the development of the zymotic poison in this locality, which has never again suffered from any such invasion, the faulty sewer having, of course, been repaired.

V. That the habitual introduction of miasmatic effluvia into the system, by *pulmonary* absorption, has a like effect in determining the active development of zymotic poisons in the bodies of those who are subjected to their influence, appears to be no less conclusively proved by the evidence now so abundantly accumulated in regard to the "seats of election" of epidemics. Our cholera reports teem with examples of this kind. The fearful outbreak at Albion Terrace, Wandsworth—in which, without any other ostensible cause than abominably bad sewerage, and, possibly, contaminated water, with a special accumulation of filth in the house first attacked, no fewer than 42 persons were attacked with cholera, and 30 died, out of a population of 120, inhabiting seventeen roomy, comfortable dwellings—will be in the recollection of most of our readers.* We may also remind them of the case of Witham, a suburb of Hull,† in which there was an accumulation of night-soil and other offensive rubbish in a triangular space of about three acres, which had been represented to the local authorities as almost certain to induce a severe outbreak of cholera; the disregarded prediction was most fearfully verified by the occurrence of no fewer than 91 deaths

* Vol. vii. p. 22. † Vol. vii. p. 17

in the immediate neighbourhood,—a greater number than was observed in any other open area within so limited a circuit. That the invasion, not only of cholera, but of other zymotic diseases, is determined by the habitual respiration of an atmosphere charged with miasmatic particles, is sufficiently obvious from the fact, that most of the localities in which cholera raged with peculiar intensity, had previously acquired an unenviable notoriety as "fever-nests;" and that they were remarkable, too, for the severity of exanthematous diseases, and for the prevalence of such affections as cancrum oris, laryngismus stridulus, and others which are now coming to be traced in great degree to similar predisposing causes. The case of the Whitechapel Workhouse, formerly cited by us* from the Report of the Board of Health on Cholera, is a very striking example of this general fact; and the evidence afforded by the Potteries at Kensington most strikingly accords with this; the locality being distinguished above almost every other by the intensity of its "piggishness" (to use the most expressive term we can think of), and also enjoying the unenviable notoriety of being more unfavourable to human life, and especially to infantile life, than probably any other locality in this country.†

VI. It might not seem at first sight apparent, in what way the condition of persons who are suffering under the simple *privation of food*, is allied to that of individuals who are subjected to the introduction of decomposing matters into their blood *ab extra*, or who possess *within themselves* some unusual source for the generation of like substances. Yet nothing is more certain in the whole range of etiology, than that famine is one of the most powerful among the conditions predisposing to pestilence, and that it is also most constant in its operation; so that, whenever we hear of a famine, we look for fever or some other epidemic as its almost necessary sequel. Now it has been commonly supposed that the lowering of all the vital forces by deficiency of food, constitutes the particular condition which renders a starved population so peculiarly open to the invasion of zymotic diseases; but we think that there is something far more potent than this. It is one of the most curious phenomena of starvation, yet one of which it is not easy to give a satisfactory explanation, that a state of general putrescence supervenes even during life; as if the want of material for the generation of new tissue, were an obstacle to the deportation of that which has become effete. Upon this point all observers are agreed. Thus, Dr. Donovan, in his account of the Irish famine of 1847,‡ speaks of the fœtid odour exhaled from the skin, which is itself covered with a brownish, dirty-looking, and offensive secretion. The general tendency to decomposition is further evidenced by the rapidity of post-mortem decay, an absolute putrescence being often apparent before the extinction of life. And the colliquative diarrhœa, which is so frequently the immediate cause of the fatal termination, by its weakening effect, not merely in cases of inanition, but also in the subjects of exhausting diseases, may probably be considered as a manifestation of the general disintegration of the system. When these facts, then, are taken into the account, does it not seem next to certain, that the presence of a large amount of decomposing matter in the system, which is not adequately eliminated by the excretory processes,

* Vol. vii. p. 16. † Vol. vii. pp. 18, 19; and vol. ix. pp. 146, 147.
‡ Dublin Medical Press, Feb. 1848.

here also affords the condition most needed for the development of the zymotic poison?

VII. An accumulation of disintegrating matter in the system may be due, not merely to its excessive production, but to any obstacle which interferes with its due elimination; and this will be especially the case, when the respiratory process is imperfectly performed. All physiological and pathological evidence tends to indicate the paramount importance of this process; not merely as regards the direct elimination, through the lungs, of a large amount of matter which is undergoing change; but also as furnishing the conditions, by which the matters properly to be excreted by the kidneys, are brought into the normal condition for being thus eliminated. For any prolonged deficiency of respiration necessarily lowers the general oxidating process throughout the body; and thus it happens that an undue amount of carbonaceous matter is thrown upon the kidneys for excretion, and that the highly-azotized compounds are not so completely brought, as they should be, into the condition of urea. Now, that overcrowding, and consequently deficient aëration of the blood, is one of the most frequent causes of the severity of epidemics, is a fact so universally recognised by all who have attended to the subject, that we need scarcely do more than advert to it. The cases of Kurrachee,* Bellary,† Taunton workhouse, Tooting, and Millbank prison,‡ formerly referred to by us, are most pregnant proofs of the influence of a limited supply of air in producing a liability to the invasion of cholera; and what is true of cholera is true also, there is every reason to believe, of other zymotic diseases. Valuable evidence to this effect is furnished by the experience of some of our Indian military stations; for it may be generally predicated of these, that whenever they are distinguished by an unusually high rate of mortality through a long series of years, and this excess is not attributable to any local causes of endemic disease, it is occasioned by insufficient supply of air. On this point we would refer our readers to the facts stated in the article on "Tropical Hygiène," contained in our fifth volume; but would here remind them, that whilst the average annual mortality of European troops under favourable circumstances does not much exceed 30 per 1000, this may be increased to 75 or even 100 per 1000 by insufficient barrack-accommodation; and that the average mortality in the gaols under British control in India is actually not less than *one in ten*, rising in some instances to *one in four*, the average allowance of air in these miserable dens being no more than 300 cubic feet (from 800 to 1000 cubic feet being provided in all well-constructed gaols in this country), whilst in some instances it is no more than 70 cubic feet. It is a fact not a little confirmatory of the doctrine here advocated, that of the twenty-three individuals who survived that fearful night's imprisonment in the Black Hole of Calcutta, which proved immediately fatal to 123 persons, several died very shortly afterwards of "putrid fever." We might refer, moreover, to the sad experience of our emigrant ships, as demonstrating the terrible influence of overcrowding and insufficient ventilation upon the propagation of fever; but it can scarcely be necessary to enter into details on this point. We would simply make a remark, however, on the well-known fact, that the sailors who navigate these vessels suffer far less severely than the passengers; not-

* Vol. ii. pp. 81—89. † Vol. iii. p. 26. ‡ Vol. vii. pp. 7—10.

withstanding that the state of their berths in the forecastle, as regards ventilation and cleanliness, is probably no better than that of the "between-decks." This seems to us fairly attributable to the fact, that the sailors never remain below for more than four hours at a time; and that, although their respiration may be partially obstructed during that period, it is so much promoted by the exposure and activity which their duties involve whilst they are on deck, that the deleterious effects of the habitual respiration of a foul atmosphere is thus averted. The emigrant passengers, on the other hand, are crowded below during the whole night, and for much longer periods during foul weather, and have no active employment when on deck; so that their respiration during the whole voyage is below the normal standard.

The evil results of an insufficient supply of air are not exerted merely through the imperfect oxidation and elimination of the substances which are undergoing decomposition within the system; for the same cause will operate to confine the putrescent effluvia that are given off *as such* from the lungs and skin, which will produce the same effect upon the individuals habitually exposed to them, as if these were generated from some external source. It was ascertained by the experiments of Collard de Martigny, that the fluid exhaled from the lungs is by no means pure water, but contains as much as three parts in 1000 of organic matter. If this fluid be kept in a closed vessel, and be exposed to an elevated temperature, a very evident putrid odour is exhaled from it; and from the recent experiments of Mr. R. A. Smith,* it appears that its putrescence depends on the decomposition of an albuminoid substance. There is every reason to believe that the fluid exhaled from the skin is charged with a very similar substance; its presence being indicated by the foul odour of garments that have been too long worn. And thus imperfect ventilation becomes the means, not only of preventing the due elimination of decomposing matter from the body, but actually of re-introducing its poisonous products into the blood, by the very process which was designed for the purification of the vital fluid. It seems, moreover, quite legitimate to conclude, that when the normal oxidation of the decomposing matter is prevented, the amount of that matter excreted in a state of imperfect oxidation will be increased; just as a lamp or fire smokes, when there is not enough air to support a perfect combustion. This view derives strong confirmation from the experiments of Professor Liebig, who has shown that putrescent compounds which give the peculiar character to fæcal discharges, may be artificially generated by the imperfect oxidation of albuminous substances; whilst Städeler has recently obtained from the extractive of the urine of the cow, a set of products bearing a remarkable analogy to those produced by the imperfect oxidation of organic compounds in the process of destructive distillation, one of them being actually identical with the carbolic acid of soot.

Of the tendency of the habitual respiration of an imperfectly-renewed atmosphere, already despoiled of part of its oxygen and charged with carbonic acid, and also loaded with the miasmatic emanations of the bodies which it supports, to produce a peculiar liability to zymotic diseases, we have a pregnant instance in the condition of the Icelandic population,

* Philosophical Magazine, vol. xxx. p. 478.

formerly noticed by us (vol. v. p. 456), whose susceptibility to epidemics is scarcely less extraordinary than that of the Färoe islanders; the state of the system, in our apprehension, being essentially the same in both cases, although induced, in the one, by the retention within the body of the products of its own decomposition; and, in the other, by the introduction of similar products as ordinary constituents of the food.

VIII. Among the predisposing causes which promote the spread of epidemics, *intemperance* is admitted to hold a high rank. Its efficacy has been generally attributed to the general disorder of the nutrient process, and to the weakening of the vital powers, which it tends to induce; but to us it appears that it possesses a more direct and special action. One of the best-established among the consequences of the introduction of alcohol into the blood, is its rapid oxidation, whereby it is itself eliminated from the circulating current: but, in thus greedily appropriating to itself the oxygen which the respiratory process supplies, it *prevents* the oxidation of other substances, of which it is one of the special objects of that process to get rid; thus tending to induce the same condition of the blood, as that which is consequent upon obstructed respiration. And the peculiar potency of this cause in hot climates, where the oxidating process, as measured by the production of carbonic acid, does not take place at above half the rate at which it is carried on in a colder atmosphere, is a strong confirmation of this view. The experience of the whole medical service of India is in accordance upon the fact, that fever, cholera, dysentery, &c., are peculiarly prone to attack the intemperate, and are peculiarly severe and fatal among the individuals attacked; and the evil is greatly aggravated, when the activity of the respiratory process is further diminished by overcrowding or insufficient ventilation. Thus, the 63rd regiment, which had not been remarkable for sobriety, lost 73 men during a nine-months' sojourn at Secunderabad, or at the rate of nearly 79 per 1000 for the whole year; the mortality of all the other stations in the Madras command being at the rate of 30·2 per 1000 for the same year. But when this regiment was replaced at Secunderabad by the 84th, a large proportion of which consisted of total abstainers, whilst nearly the whole remainder were habitually temperate, the mortality of the station was reduced to 34·2 per 1000, which *for it* was unprecedentedly low, being less than half the average of that station for the fifteen years preceding, and absolutely below that of the remainder of the Presidency for the same year. Now the mortality of the 84th regiment during the preceding year, whilst quartered in Fort St. George, had been but 12·1 per 1000; so that as we have no reason to believe any other causes than those now assigned, to have been in operation in either case, we may regard the increase of mortality in that regiment from 12·1 to 34·2 per 1000, as due to the imperfect barrack-accommodation at Secunderabad, whilst the difference of 47 per 1000 between the temperate 84th and the intemperate 63rd seems fairly attributable to the effects of alcoholic excess, aggravated by imperfect respiration.

IX. The fatigue resulting from excessive muscular exertion, moreover, is commonly accounted one of the predisposing causes of zymotic disease; and this too is usually supposed to operate merely in occasioning a general depression of the vital powers. But here too we trace a more definite and

direct connexion between the cause and its effect. All muscular exertion, it is now universally admitted, involves as its condition a disintegration of muscular tissue, the components of which normally undergo oxidation, so as to be partly eliminated by the respiratory process under the form of carbonic acid and water, and partly by the kidney under that of urea, &c. Now if the disintegration of muscle by exercise take place faster than the matter thus set free to decompose can be oxidated and eliminated, it must remain in the blood for a time, in that very state of readiness to change which renders it peculiarly fermentible; and thus its presence in the circulating current will give to the blood the same susceptibility to the action of zymotic poisons, which it will derive from any of the causes already mentioned. And that this is the true *rationale*, seems to us to be strongly indicated by the fact which lies within the experience of every one, that muscular exertion so much more speedily induces fatigue in a warm atmosphere than in a cold one; and also by the fact, which is a matter of common notoriety in India, that troops on a march are peculiarly liable to suffer from fever, cholera, dysentery, or other zymotic diseases. For in proportion as the oxidating process is reduced in activity by a high external temperature, in that proportion must the products of the muscular disintegration tend to accumulate, inducing the feeling of fatigue by the deterioration of the blood to which their presence gives rise; and in like manner occasioning an extraordinary fermentibility in the circulating fluid, which renders it peculiarly ready to undergo a morbid change when any zymotic poison is introduced into it. Now if this view be correct, we should expect to find the predisposing power of muscular exertion peculiarly augmented, either by any obstacle to free respiration, or by the ingestion of alcoholic liquors which will diminish the efficacy of that respiration. And such is undoubtedly the case. The careful analysis which we formerly made of the causes of the terrible fatality of the cholera at Kurrachee, brought into the strongest possible relief the similar potency of insufficient respiration and of excessive muscular exertion, taken separately, and their terrible increase of power when combined: the details of this analysis we need not at present recapitulate; but its general results are so strikingly brought out by a tabular arrangement of them, that we cannot refrain from presenting them anew under this form.

Designation.	Strength.	Deaths.	Deaths per 1000.	Exposure at Drill, &c.	Provision for Respiration.	Previous Exertion.
Officers' ladies	42	0	0	Nil	Good	Nil.
Officers	200	3	15	Ordinary	Mostly good	Nil or slight.
Horse Brigade	135	5	37	Ordinary	Good	Moderate.
60th Rifles	980	75	76·5	Ordinary	Bad	Nil.
Artillery	375	37	96·6	Ordinary	Good	Severe.
Bombay Fusiliers	764	83	108·6	Ordinary	Very bad	Nil.
Soldiers' wives	159	23	144·6	Nil	Mostly very bad	Partly severe.
Do. of 86th Regt.	...	...	166·6	Nil	Very bad	Very severe.
86th Regiment	1091	238	218	Ordinary	Very bad	Very severe.
	3746	464	124			

Thus we see that the *highest* rate of mortality presents itself where the three causes referred to—namely, the fatigue and exposure of drill, the imperfect ventilation, and the previous exertion in a long march—were in concurrent action; the *absence* of mortality where neither of them was in operation. The difference between the mortality of the Bombay Fusiliers and that of the 86th regiment, which were under precisely the same circumstances as regards exposure and ventilation, shows the marked influence of previous exertion; but that this would not itself account for the high rate of mortality in the 86th, is shown by the far smaller proportion of deaths in the Artillery, three out of the four battalions of which had been subjected to the same previous fatigue, but were well accommodated at Kurrachee, so that their mortality was less than that of the Bombay Fusiliers who had not marched at all, but were overcrowded in ill-ventilated tents. The condition of the soldiers' wives again would be much the same as that of their husbands, as regards accommodation and exposure during a march; on the other hand, they are exempt from the fatigue and exposure of drill; and accordingly we observe that whilst the officers' ladies escaped altogether, the soldiers' wives were affected nearly as severely as their husbands, the loss being the greatest among the women of the unfortunate 86th.

So, again, when the influence of a march in producing an unusual amount of muscular disintegration, is added to that of alcoholic excess in retarding the elimination of the decomposing products, and the march is performed under a high external temperature, which tends to diminish the activity of the respiratory process, we should expect that the liability to zymotic disease would be peculiarly augmented; and of this we have a marked example in the marches of the 84th Regiment, from Madras to Secunderabad, and of the 63rd, from Secunderabad to Madras, performed at the same time, through a pestiferous country; for whilst the former did not suffer at all from cholera or fever, and only lost two men by dysentery (both of whom were old chronic cases taken out of hospital at Madras), the latter lost a considerable number of men on the road, and had so many sick when it met the 84th, as to be obliged to borrow its spare *dhoolies* or sick-palanquins.

Thus, then, if we bring together the results of our examination into the *modus operandi* of the generally-recognised predisposing causes of zymotic disease, we find that they are all reducible to one of three categories:—(1) Those, namely, which tend to introduce into the system decomposing matter that has been generated in some external source;—(2) Those which occasion an increased production of decomposing matter in the system itself;—and (3) Those which obstruct the elimination of the decomposing matter normally or excessively generated within the system, or abnormally introduced into it from without.

1. Under the first head will rank putrescent food, water contaminated by sewerage or other decomposing matter, and air charged with miasmatic emanations.

2. Under the second, any unusual source of degeneration of the tissues within the body, such as presents itself in the puerperal state, after severe injuries, or as a consequence of excessive muscular exertion.

3. Under the third, an insufficient supply of air, a high external temperature (which slackens the respiratory process), and the ingestion of alcohol.

We cannot assign a definite place to starvation, until we more clearly know the cause of the retention of the decomposing matter within the body.

Any one of these causes will tend to produce *an accumulation of disintegrating azotized compounds, in a state of change, in the circulating current;* and this is precisely the condition which, on the fermentation-theory, will afford the greatest readiness to the development of any zymotic poison in the system, whatever the specific nature of that poison may be. But as each of these causes will operate separately, their potency is vastly augmented when they act concurrently; as when the puerperal patient, whose special liability arises from the source of disintegrating matter within the system, is also subjected to putrescent emanations from some external source, and is pent-up in a heated atmosphere, very insufficiently renewed. Let any poison, capable of engendering puerperal fever, be applied to a female thus circumstanced, and our readers will not have much doubt about the fatality of the result. Or, again, if to the influence of the fatigue and exposure of a long march in a tropical country, be added that of overcrowding in tents or barracks, and on that be superimposed the ingestion of putrescent water, or of alcoholic liquors in excessive amount,—and if the individuals, thus primed, be exposed to the poison of cholera, dysentery, or fever,—the experience of our Indian army tells a fearful tale of the consequences of such a combination.

Looking at the question, then, in the first place, as one of experience simply, we believe that no one who duly weighs the evidence, can hesitate to admit the facts on which our superstructure is based. Looking, secondly, to the immediate consequences of each of the causes we have enumerated, we believe that no physiologist can hesitate in admitting the possibility, to say the least, of the *rationale* which we have assigned for every one. And when we find that each of these causes, without a single exception, tends to induce *one and the same condition of the blood,* the conclusion seems to us almost irresistible, that this condition of the blood,—in which it is charged with decomposing organic compounds,—is that which is the immediate source of its liability to be affected by the introduction of zymotic poisons. All this is a matter of direct *induction;* and the only hypothetical part of the doctrine, is the occurrence of anything like fermentation as a consequence of that agency. Here we consider that our doctrine affords to the fermentation-theory a support of which it stands greatly in need; for if experience demonstrate, that zymotic poisons have little morbific power over blood which is exposed to them in a state of purity, but immediately exert their malign influence when the blood is unduly charged with organic matter prone to change, the *modus operandi* of the poison acquires so remarkable an additional feature of resemblance to that of "ferments," that the essential correspondence of the two seems to us almost indubitable.

If it be inquired, what is the practical bearing of this discussion, we at once reply that, if our view be correct, it would be possible to extinguish the greater number of epidemic diseases, however intense or abundant may be the atmospheric or other agencies which constitute their potential

causes, by preserving the blood of every individual in that state of unfermentibility (if we may coin such a word), which shall effectually prevent these poisons from finding the conditions of their development within the body; this end being to be attained, on the one hand, by preventing (so far as may be possible) any unusual production of fermentible matter in or out of the body; and on the other, by promoting its removal when it is inevitably generated (as in the puerperal state), through the respiratory process, which ought to be favoured as much as possible, not merely by a free supply of air, but by the reduction of that air to the lowest temperature at which the condition of the patient will allow it to be safely inhaled. We have no new measures to propose; but we do think, that by more clearly specifying than has heretofore been thought possible, the *rationale* of those of which experience has demonstrated the value, we may promote their employment, not only with greater positiveness and consistency, on the part of the profession, than has yet been manifested by the majority of them, but also to a far wider extent than that to which they have hitherto been carried out.

William B. Carpenter.

REVIEW XV.

Das Epithelioma, eine eigenthümliche Geschwulst, Die Man im Allgemeinen bisher als Krebs angesehen hat. Von ADOLPH HANNOVER. Nebst zwei Lithographirten Tafeln.

Epithelioma, a peculiar Tumour, which has been hitherto generally regarded as Cancer. By ADOLPH HANNOVER.—*Leipzig.* 1852. 8vo, pp. 149. With Two Lithographs.

As a provisional term, to be employed until, in the progress of research and with the results of more close diagnostic acumen, we may be enabled to separate from each other lesions bearing many strong points of mutual resemblance, the word "cancroid" has been and is of essential service to the pathological observer. It embraces a group of lesions which have been for a long time confounded with cancer, and forms a very convenient heading under which to place certain morbid specimens, of which we are in a position to state that they are not cancer, but of whose real pathological nature and exact nosological place we are still uncertain. With the advance of micrologic inquiry, however, still greater precision becomes requisite, and we find the term cancroid too vague to satisfy the necessities of a strict pathological anatomy.

Diseases hitherto associated by the common feature of an absence of the general or anatomical characters of cancer, require to be no less carefully distinguished from each other.

M. Hannover's memoir takes up the consideration of one very important subdivision of this class of diseases—namely, those tumours whose chief constituent element is epithelioma in a more or less modified form, and which occur on surfaces naturally covered with a layer of epithelial cells. For this class of tumours he proposes the name of epithelioma; we shall have to consider, in the course of our analysis, how far he is warranted in

forming these tumours into a separate order, and how far we are justified in accepting this new addition to medical nomenclature.

The term epithelioma is applied by our author to those tumours which occur on surfaces provided with a perfect pavement epithelium, as the skin and mucous membrane; and he states that no observation has been hitherto recorded of their occurrence on mucous surfaces provided with only cylindrate or ciliate epithelium, or in the interior of organs, or in shut cavities not furnished with a complete pavement epithelium. The vast majority of epithelial tumours present themselves on the mucous membranes, but three remarkable examples are on record of their occurrence on serous surfaces. M. Lebert has observed one instance of an epithelial growth on the internal surface of the parietal layer of the arachnoid membrane, which consisted of little, soft, red tumours, composed of vessels and epithelial cells, presenting, in all respects, the characters of the epithelium of this serous membrane.* M. Robin has seen an epithelial tumour on the inside of a vein in the bone, while Rokitanski has given a case of an epithelial tumour of the liver, which was invested with a fibrous capsule.

The principal seats of epithelioma are the vicinities of the great orifices, the mouth, nose, anus, and those of the male and female sexual organs. The lips, and more especially the under lip, are particularly liable to the formation of epithelial growths; and, according to M. Hannover, the number of cases which occur in the latter situation exceeds that of all the cases which present themselves in other parts of the body. Next to the lip, in order of liability to the disease, come the tongue, the male and female sexual organs, the several parts of the head, the trunk, and limbs; it is least often found on the inner surface of the cheeks, in the jaw-bones, the larynx, the cardiac orifice of the stomach, and the anus.

The disease commences as a small induration superficially covered by the epidermis or epithelium, having its seat in the rete Malpighi, and being sometimes accompanied by a pustule or an excoriation. The chorion also becomes implicated. In proportion to the extent to which each of the constituents of the skin, the chorion, rete, and epidermis enters into the formation of the growth, will be its further development, and the appearances which it presents superficially and on section. Thus, the tumours will be found mulberry-shaped, villous, of a cock's-comb or cauliflower appearance, or they may resemble a collection of pedicled condylomata. The surface of the tumour may, in its smallest divisions, remain for a long time covered with epidermis; but in many cases the surface becomes ulcerated and excoriated at an early period, and an actual loss of substance takes place, especially towards the centre of the growth, where an ulcer forms with an irregular, grey, often bloody base, while its borders are elevated and undermined, and are generally covered with crusts of the fœtid and ichorous discharge. If cut through in its early stages, the tumour presents on the surface of section very well-defined borders between the epidermis, which is frequently several lines in thickness, and the rete, and between the latter and the chorion, which is also hypertrophied. The latter division is indicated most clearly by the undulating outline of the papillæ prolonged from the chorion. The entire substance is friable, breaks asunder readily, and can be separated from the surface of the chorion,

* See Lebert, Traité des Maladies Cancereuses, &c., p. 758.

from which the papillæ spring. When the epithelial formation has struck root more deeply into the chorion, it presents a white-and-grey sprinkled surface, in which is found a dense basement layer or stroma, wherein a variable quantity of whitish bodies may be seen, varying in size from a point to a millet-seed; this appearance, according to M. Hannover, has in many cases led to the confounding of epithelioma with reticulated cancer. The white granules can be easily removed from the grey stroma, in the form of cylindrical bodies many lines in length. The grey substance is in general firm, the white masses being soft or even semifluid; the quantity of both varies not only in different tumours, but even in different parts of the same tumour, which may thus be in one place extremely hard, and in another very soft. When the papillæ of the chorion become elongated, the pedicles of the condylomata at the same time being implicated in it, so that all the constituents of the skin are mixed up together, the tumour assumes a fibrous aspect. From the freshly-cut surface, when scraped with a knife, a milky, granular fluid is obtained, which when mixed with water does not give the semitransparent, opalescent emulsion which is produced by the mixture of the *succus* of cancer with water. On the contrary, the mass mixes with difficulty, as if it contained fat. This characteristic is forcibly dwelt on by Lebert, who considers that there is no physical property of cancer appreciable by the naked eye, of such value as that furnished by careful examination of the *succus.* As the best means of obtaining it pure, he recommends gentle compression of the cancerous tissue; this proceeding causes a number of little drops to exude, which can be then transferred, by the point of a scalpel, to a glass slide; if the drop be then covered with a thin slide, it still presents a homogeneous appearance, but acquires a certain amount of transparence by compression. On the addition of a small quantity of water the mixture becomes a little less troubled, but remains equally homogeneous. Cancer-cells thus form an emulsion with water, and this becomes an important characteristic, for if water be added to a liquid which does not form an emulsion, a grumous appearance results, leaflets or irregular masses being distributed through the liquid, which may remain almost limpid in the intervals. Thus, tuberculous matter mixed with water becomes separated into a multitude of little clots. The division into leaflets takes place principally in epidermic cancroids (epithelioma), and the distribution into more regular masses is observed in the pseudo-succus of the hypertrophy of the mammary gland, and in certain fibro-plastic tumours.* Whatever value we place on these observations, and they are certainly entitled to great weight, the diagnosis cannot be considered complete until the morbid specimen has undergone a careful microscopic examination.

Into the composition of an epithelioma enter the elementary constituents of the skin or mucous membrane, only modified in form, and in their mode of arrangement.

Thus the epidermic or epithelial cells will be found to enter, not only into the thickened superficial layers and detached crusts, but also into the interior of the wart-like bodies, and into the roots which have struck into the epidermis.

We thus encounter cells in all parts of the tumour, in the deepest as well as in the most superficial layers; and as they sometimes constitute

* Consult pp. 6, 7, 8, of Lebert, op. cit.

nearly the entire of the growth, it is evidently on the microscopic character of the cells found in any particular tumour that the question of diagnosis turns. MM. Lebert and Hannover are both strenuous advocates of the specific nature of the cancer-cell, and agree pretty closely as to its chief diagnostic features. In order to establish a standard of comparison, we will first give a brief exposition of the most recent results arrived at with regard to the microscopic characters of cancer; with these we shall then contrast the microscopic appearances of tumours most liable to be confounded with cancer, more especially the epitheliomata; and we shall then give a succinct resumé of M. Hannover's researches on this peculiar class of growths.

Under the head "Microscopic Characters of Cancer," M. Lebert* has entered with considerable detail into the description of the microscopic elements of cancer. He dwells with great force and distinctness on the cell which he calls the "specific element of cancer."

The cell in cancer is frequently found in an incomplete condition, the perfect cells bearing, in many tumours, but a very small proportion to the number of free, large nuclei, with voluminous nucleoli. When the cell is unbroken, it presents a certain fixity of dimensions, the true mean being according to M. Lebert, from $0{\cdot}02^{mm}$ to $0{\cdot}025^{mm}$; in very rare cases, this dimension may be increased to $0{\cdot}04^{mm}$ or diminished to $0{\cdot}015^{mm}$.

The type of the cancer-cell is, according to the same author, a small regular sphere, with an excentric elliptical nucleus occupying half, or even more, of the interior of the cell, and enclosing one or many large nucleoli. This type, however, says M. Lebert, is but seldom pure, and in fact a very marked feature of cancer-cells is the *multiformity* of their walls. The nucleus is an exceedingly important and constant element in the cancer-cell, its predominant type is the ovoid or elliptical form, its borders being strongly marked. Its mean size is from $0{\cdot}01^{mm}$ to $00{\cdot}15^{mm}$; it may reach $0{\cdot}02^{mm}$ or descend so low as $0{\cdot}0075^{mm}$. The nucleolus is large, dark, and much more voluminous and striking in appearance than in any other kind of cell; its mean dimensions vary between $0{\cdot}0025^{mm}$ and $0{\cdot}0033^{mm}$. There may be as many as three nucleoli, but there are seldom more.

Under rare circumstances, the nucleolus may reach a size of $0{\cdot}004^{mm}$, or $0{\cdot}005^{mm}$, and may then present one or more large molecules in its interior.

Grouping these characters together, we obtain a series of diagnostic elements not presented by cells of any other kind—viz., fixed mean dimensions, as above, multiformity of the cell-wall, a nucleus voluminous in itself as well as in relation to the cell which surrounds it, and lastly, a large dark, and very apparent nucleolus. Placed in contrast with this class of cells, those normally found on the epidermis, or any of the mucous surfaces, or developed in any of these situations as the results of diseased action, will be found to present very marked and striking differences; in fact, in not one of the above characteristics will there be found a strict accordance between cancer-cells and those of any epithelial surface.

We shall now turn to the study of the microscopic elements of the

* Op. cit., p. 15, et seq.

epitheliomata, as developed by M. Hannover. The cells found in these tumours are all more or less modified forms of those normally found on the surface which is the seat of disease. They consist of cell-membrane, contents, nucleus, and nucleolus. The cell-membrane is capable of assuming various forms; it may be oval, circular, or, in situations where the cells are pressed closely together, of an angular form; the cell-wall is very thin, so that it is frequently rolled up, or thrown into thin folds, whereby the cell assumes a striated appearance. The fine transparent, sometimes granular contents, which are best seen when the cell is made to roll over, fill the entire cell when it is round or oval; but if it be angular, they are generally collected in particular places, often in the middle of the cell, or around the nucleus. When the contents escape, the borders of the cell collapse, and the cell-wall appears as a flat irregular plate or table. Sometimes the folds of the cell-wall are so fine that the cell appears to have split into fibres; but even in this state, by the addition of weak acetic acid, the cells become softened, the whole preparation appears clearer, and the nuclei become visible.

The nucleus is small in relation to the cell (compare, *supra*, the cancer nucleus); it is round or oval, often pointed, cordiform or irregular; it is finely punctated, and may contain separate large granules; in some cases it presents a double contour, or, again, it may be surrounded by a clear ring. The nucleus may be absent in large as well as in small cells, or its place may be supplied by a mass of fine granules having the appearance of fat drops. Two or three nuclei are occasionally met with. The nucleolus is seldom clear; one large or many small ones are to be found in different cases. These characters, it will be observed, differ remarkably from those above assigned to the cancer-cell. In the case of an imbedded cell, which is occasionally met with, it will be necessary to attend to the form and appearances of both the containing and the contained cell, as otherwise they might be mistaken, on superficial examination, for a cancer-cell, with large nucleus. The occurrence of a doubtful isolated cell amidst others which present unmistakable evidence of an epithelial origin, cannot lead to any practical difficulty. The epithelial cells exhibit the property of becoming elongated at one or two sides; they thus will be found to present caudate, spindle-shaped or club-like forms.

As long as the tumour remains smooth, the disposition of the elementary parts is not notably changed; the rete Malpighi has generally the greatest proportionate thickness, its cells are clear, and especially on the addition of dilute acetic acid. The youngest and deepest have the smallest diameter, but possess larger nuclei than the completely developed cells. When the tumour has a villous surface, each little cylinder will be found to consist of an atrophied papilla of the chorion, containing a small quantity of areolar tissue, and a vascular loop; the chief mass of the papilla, however, is formed of epithelium. When the papillæ are more fully developed and wart-like, the epithelial cells become prolonged, and from their longitudinal arrangement cause the fibrous or striped appearance which is seen chiefly in the pedicles of the warts, but this may be present in more superficial parts also. When the epithelial formation has invaded the chorion, the gray stroma already spoken of will be found to consist partly of areolar

tissue and elastic fibres, and partly of the cells of the rete Malpighi, which have become incorporated with the elements of the chorion. The little isolated bodies which can be pressed out of the mass consist of epithelial cells aggregated together in concentric layers; if treated with acetic acid the cells may be separated from each other, and then become apparent as such.

Besides the part which the chorion takes in the formation of the gray stroma, it also constitutes the basis from which the tumour springs, and it will consequently be found more or less hypertrophied and inflamed. Its vessels increase in number, blood frequently escapes, and the ichor secreted may be mixed with blood. As the blood generally comes from the base of the ulcer, or from the deep furrows between the warts and papillæ, it is more probably derived from the vessels of the chorion itself than from those of the papillæ. In the examination of epitheliomata we must be prepared to meet with different extraneous elements, such as the molecular masses or *débris* of muscles and bones, fat cells, oil drops, &c. Crystals (cholesterine frequently), vibriones, and minute vegetable formations, likewise occur.

If we now compare these results of the microscopical examination of an epithelial tumour with those already detailed as characteristic of cancer, we may at once perceive that the resemblances are only superficial, the differences sufficiently marked and striking to enable us to distinguish between the two forms of disease without much difficulty. M. Hannover speaks, however, of a condition of things in which it is much more difficult to draw the line of distinction; we allude to the combination of cancer with epithelioma in the same tumour. Our author is by no means an advocate of the now exploded doctrine of the transformation of tissues, but he recognises the possibility of a cancerous deposit taking place in an epithelioma already formed, which he considers to present just as favourable a locus for the development of cancer as any other indifferent situation. He speaks of having observed such a case; without denying the possibility of its occurrence, we can only say we have ourselves no experience of such a combination.

Having thus studied the microscopic characters of epithelial tumours, and having seen by contrast how they differ from those of cancer, the question arises as to how far it is expedient or coexistent with the present state of our knowledge, to separate this class of tumours from others at present included with them under the term cancroid, and how far it is necessary to admit an addition to our nomenclature for the purpose of more distinctly specifying the order of growths under consideration.

In wholly rejecting the word cancroid, which, as we have already observed, forms such a convenient heading under which to range many nondescript tumours frequently confounded with cancer, but capable of being separated from it by accurate and now established principles of diagnosis, we should experience much inconvenience in dealing with these formations, which, though differing much from each other, have the one feature in common of being certainly not cancerous, though liable to be confounded with cancer. It may be well, therefore, to retain the term until to each of the several affections now grouped together under the name of

cancroid be assigned a distinct and proper place in the category of tumours, and be appropriately named. That such a classification and nomenclature is required by the strict rules of pathological anatomy as now cultivated must be admitted; and as an advance in this direction we are decidedly of opinion, that the term epithelioma should be received and adopted. The group of lesions which it comprises have all special characters which separate them from other non-cancerous diseases. Thus they differ widely from the fibro-plastic, the fibrous, and the proper glandular, non-cancerous tumours. Amongst themselves they present a uniformity of structure and of pathological relations. They are therefore entitled to be withdrawn from the vague group of cancroid diseases; and we agree with M. Hannover in considering that they deserve to be ranked as a separate family, and to receive a distinctive name. Perhaps until some other observer shall have effected as much for the precise diagnosis and pathological history of the other non-cancerous affections, it will be well to retain the term cancroid to group them together, it being always understood that this term is retained and used provisionally.

The special history of epithelioma, as it occurs in different parts of the body, occupies a very considerable portion of M. Hannover's memoir. The several sections contain much interesting critical and historical matter as well as original observation, and will be read with interest. We may observe, in conclusion, that this class of tumours has received much elucidation from the labours of M. Hannover; and that our knowledge of them has been rendered more precise and definite by the introduction of an appropriate term under which to group them.

Robert D. Lyons.

Review XVI.

1. *A Treatise on Tuberculosis, the Constitutional Origin of Consumption and Scrofula.* By Henry Ancell, late Surgeon to the Western General Dispensary, &c. &c.—*London*, 1852. 8vo, pp. 779.

2. *The Nature, Symptoms, and Treatment of Consumption; being the Essay to which was awarded the Fothergillian Medal of the London Medical Society.* By Richard Payne Cotton, M.D., Member of the Royal College of Physicians, and Assistant-Physician to the Hospital for Consumption and Diseases of the Chest.—*London*, 1852. 8vo, pp. 286.

3 *Tuberkulose und ihre Beziehung zur Entzündung, Scrophulosis, Typhus.* Von Virchow, Phys.-Med.—*Würzburg*, 1850. *Band* I. pp. 82.
   Zur Geschichte der Tuberculose. Von Virchow, Phys.-Med.—*Würzburg*, 1851. Band II. pp. 70.
   Ueber die Verschiedenheit von Phthisie und Tuberkulose. Von Virchow.
   The non-identity of Phthisis and Tuberculosis. By Virchow.—*Würzburg*, 1852. Band III. pp. 98.

4. *Handbuch der rationellen Pathologie.* Von Henle.
   Manual of rational Pathology. By Henle.—1850. Band II. pp. 784.

5. *Ueber die Uebereinstimmung der Tuberkelablagerungen mit den Entzündungsprodukten. Annalen der Charité-Krankenhauses zu Berlin.*
The Identity of Tuberculous Deposits with the Products of Inflammation. By B. REINHARDT.—*Berlin.* Band I. pp. 362.

6. *Benno Reinhardt's pathologisch-anatomische Untersuchungen, nach seinem Tode zusammengestellt und herausgegeben* von RUD. LEUBUSCHER.
B. Reinhardt's Pathologic-Anatomical Researches. Collected after his death, and Edited by RUD. LEUBUSCHER.—*Berlin,* 1852. 8vo, pp. 144.

WHAT is tubercle? What is its anatomical constitution? What is the pathological process by which it is formed? How is tubercle to be defined, so that all may signify the same body by the same name? What is phthisis pulmonalis? Do all writers using that term intend by it the same disease—i. e., the same anatomically and pathologically? Are many diseases confounded as one under the name phthisis pulmonalis? Curious it is that we should have to ask these questions at the present day. Strange that pathologists, renowned for the accuracy of their observations and for the soundness of their reasoning, whose earnest desire is to see what is, and whose anxious wish is to interpret aright what they see, should give different answers to these questions. Strange that after the labour bestowed on the investigation of these subjects by the most eminent pathologists, from Morton and Bayle to Rokitansky and Lebert, we should have still to ask what is tubercle?—what is phthisis?

The opinions now held in regard of tubercle may be divided broadly into two classes; the first is, that tubercle is an exudation essentially pathological in character. "It is beyond doubt," says Rokitansky, "that tubercle is an exudation."* The second, that tubercle is merely a retrograde metamorphosis of pre-existing structures. This latter notion is strongly advocated by Virchow, in the papers before us.

The opinions referred to, however, readily admit of more minute division; and for the purpose of enabling us, in a subsequent article, to estimate what amount of the truth they respectively contain, we shall here briefly describe them under five heads.

1st. Tubercle is a specific exudation poured out under the influence of a special general pathological state; in other words, it is the local anatomical expression of a definite constitutional affection. Or, as Mr. Ancell says: "As healthy blood supplies a blastema or succus nutritivus for healthy nutrition, tuberculous blood supplied a tuberculous liquor from which tubercle is formed."

Lebert's statement, that he had discovered in tubercle a peculiar and distinctive microscopic element—a tubercle-corpuscle—appeared to give force to this view; and coinciding as it did with opinions previously entertained, was received in this country as strong evidence in favour of the favourite creed. If this opinion be correct, tubercle ranks pathologically and anatomically in the same order as cancer, there being in both a specific constitutional disease, a specific exudation, and a specific or distinctive cell.

The truth or falsehood of this view will come hereafter to be examined.

* Handbuch der Allgemeinen pathologischen Anatomie, p. 413.

2. Tubercle is a degraded condition of the nutritive material. Some pathologists, as Dr. C. J. B. Williams, refer tubercle to a "degraded condition of the nutritive material from which new textures are formed," and hold that "tubercle differs from fibrine or coagulable lymph not in kind, but in degree of vitality and capacity for organization."* Examined microscopically, tubercle contains, according to Dr. Williams, a few irregularly-shaped, shrivelled cells, with imperfect nuclei, the main substance being composed of granular or amorphous matter. "No fibres are," he says, "perceptible."

3. Tubercle is composed of the products of inflammation. Reinhardt is at once the most recent and able advocate of this opinion, and the high reputation as a microscopical observer he enjoyed among those most intimately acquainted with him, recommend his statements to our attentive consideration. Reinhardt sees in tubercle only the products of chronic and repeated inflammations. In some cases of chronic pneumonia, Reinhardt found a gelatinous fluid in the cells and interstitial tissue, containing epithelium and pus. At a later period the epithelium was in a state of fatty degeneration; the fluid was diminished in quantity; the interstitial tissue contracted; the cells lessened in volume; and, finally, a kind of cicatrix was formed. In various stages these states have been termed, respectively, gelatinous infiltration, gray tubercle, and tubercular cicatrix. In other cases of so-called yellow tubercle, Reinhardt found pus in the air-cells; the pus became thickened, dried up, and the nuclei disappeared. Shrivelled pus-cells, and not nuclei which have become free, form the so-called tubercle-corpuscles. Although Reinhardt considers that in some instances the tuberculous process arises from local causes—viz., hyperhæmia and recurrent inflammation; yet he admits that in many cases these indicate a state of dyskrasia.

4. Tubercle is composed of dead-tissue elements: such is Henle's opinion. In the lungs, he says, tubercles are bloodless, dead (nekrotesche) lobules, gorged with the dried-up elements of the epithelium or with pus, heaps of granules and granular cells, and these dead lobules continue in connexion with the sound pulmonary tissue, as a withered limb may with the trunk.

"The corpuscles," he says, "which are found most frequently and in the greatest number in miliary and crude soft tubercle, and which have generally been described as specific, are the corpuscles named by me 'elementary corpuscles,' and they belong to that variety of these which is rendered pale and dissolved by acetic acid. I have proved," he continues, "that such forms arise out of cytoid corpuscles long exposed to the air." And, further on—"The microscopic analysis renders it probable that the nucleated cells arise out of the epithelium of the air-cells; it offers no explanation, as to whether the cytoid corpuscles, the products of the development of which we find in the air-cells, arise out of bronchial mucus, or from the pus of a circumscribed inflammation, or from extravasated blood."†

Tubercle corpuscles have already been stated by Gulliver to be "effete and shrunken primary cells"—a definition which might be adopted by Henle.

These views of Henle agree in the main with those propounded, in 1843, by Dr. William Addison:‡ "A tubercle," says Dr. W. Addison, "involves

* Principles of Medicine, p. 386.

† Handbuch der rationellen Pathologie, p. 788.

‡ Transactions of the Provincial Medical and Surgical Association, vol. xi.

or includes in its substance the vesicular structure of the lungs: minute bloodvessels, lobular passages, and air-cells, are all capable of demonstration on the dissection of tubercle under a Coddington lens; the bloodvessels are no longer permeable, but their presence may be demonstrated." Tubercles themselves are composed of abnormal epithelial cells. Henle maintains that gray granulations are imperfectly-coagulated fibrine, and if they sometimes pass into yellow tubercles, cannot be considered as their first stage. He discards the idea of a specific exudation, and advocates the opinion that the first change, as far as the lungs are concerned, is coagulation of blood in, and obliteration of the vessels consequent on, defective capillary circulation, arising from imperfection of the respiratory movements.

5. Tubercles are composed of metamorphosed organized elements—a metamorphosis co-ordinate with the fatty and the waxy degenerations. This is the opinion of Virchow. His views are developed at some length in the papers placed at the head of this article; and as they contain much that is peculiar and novel, we shall enter into them somewhat fully.

To do justice to the opinions of Virchow we shall first describe what we understand him to mean, and then give his own summary of his opinions in the words he has himself used in one of the papers above mentioned.

A tubercle is composed essentially of dead tissues, the death of the part being occasioned by the accumulation of cells amid its vessels, and consequent compression of those vessels and cessation of the circulation through them. The cells which thus play so important a part in the formation of tubercle may have their origin,—

1. In the physiological cells of a structure or organ. The mode in which the increase in these cells takes place may, he says, be exquisitely perceived in the lungs. The first step in the tuberculous metamorphosis in these organs is an increase in the epithelium of the air-cells by endogenous formation. "I have seen," Virchow says, "cells with five large, oval, granulated nucleolated nuclei." Subsequently the "cells fall to pieces, a granular detritus is left, in which the nuclei remain for some time as shrivelled irregular opaque bodies, finally these also crumble, and an entirely amorphous finely granular mass remains behind." It is these nuclei, shrivelled, irregular, and opaque, which, in Virchow's opinion, constitute the tubercle-corpuscles described by Gluge and Lebert. "*They are not*," he says, "*exudation-corpuscles*." "The peculiarity of the local process lies in the tendency of the organization, and by no means in a peculiar exudation." In *lymphatic glands* affected with so-called scrofulosis, there is hypertrophy of the elements of the part through endogenous nuclei formation. The cells enlarge to five or six times their normal size, and as many as twelve pairs of nuclei may be seen in the same cell. The nuclei probably increase in number by cleavage into pairs. What share an exudation takes in this change, Virchow says, he "cannot decide." Still he maintains that tubercle is not developed exudation, but merely metamorphosed pre-existing tissue-elements—elements to which, in their primary state, the name of tubercle could not be applied; and that, consequently, the tuberculous metamorphosis is not the mark of a specific process, of a particular constitution.

2. The cells by the accumulation of which the vessels are compressed and death of the part produced may have their origin in the endogenous

development, or in atrophy of the cells of cancer, pus, or typhous matter, but not in their simple desiccation.

3. These cells may be developed in the fibrine poured out in what is termed tuberculous inflammation. Is the tubercle here formed directly of inflammatory exudation-matter? Virchow says, No: the whole mass of fibrine passes on to organization; but while "one part develops itself into uniting tissue and vessels, another forms nucleated and cellular formations, which rapidly increase by endogenous growth, so that their number at some points is very great, and the amount of the endogenous nuclei is occasionally even colossal." The subsequent steps of the process—i. e., death of the part, disruption, atrophy, shrivelling, and desiccation of the cells, are the same in all three cases.

But although all pathological and all physiological cell-growths may thus tubercularize, yet there is a local process which leads to the exudation of a material, the cells resulting from the development of which, whether they be physiological or pathological, so constantly tubercularize and lead to local death, that this may be said to be the ordinary termination of the process. This process, in the phraseology of Virchow, is tuberculosis; while scrofulosis is used by him to signify the constitutional state in which tuberculosis occurs.

To pass from the general state to the particular local lesion.

Scrofulosis is that constitutional affection which commonly leads to tuberculosis.

Tuberculosis is that local process in the ordinary progress of which there occurs an exudation of a material, nutritive or pathological, which develops into cells that tubercularize or undergo the tuberculous metamorphosis.

Tubercularization is the local process by which the metamorphosis of the elements of a part into tubercle is effected—i. e., endogenous development, atrophy, shrivelling, and desiccation of the cells.

A tubercle is formed of the detritus of the metamorphosed and atrophied cells, with the remains of the vessels &c. of the part in which they were seated.

It requires some little attention to grasp fully Virchow's meaning; and to those who have been accustomed to use the word "tuberculosis" to denote a special constitutional affection, the employment of the term scrofulosis to express this state, and the restriction of the word tuberculosis to the local changes going on in a particular part, may be confusing; but a little consideration will prevent any misconception.

With this preamble we shall allow Virchow to state his own views, in his own words:

"1. Tuberculization, the indubitably local process by which the body described by the name tubercle is formed, is not a peculiar specific exudation, but a peculiar transformation of tissue elements, such as in 1847 I described in regard of cancer under the name of tuberculous metamorphosis.

"2. The tuberculous metamorphosis is therefore co-ordinate with the fatty and the waxy metamorphosis, calcification, and atheromatous degeneration, but in no way co-ordinate with inflammation or serous effusion, and even less so with suppuration or with cancerous formation.

"3. The tuberculous metamorphosis sometimes affects newly-formed pathological tissues, sometimes the primitive, the so-called physiological tissues, and

finally, sometimes it affects both old and new simultaneously, and this last is its ordinary and peculiar characteristic. The tuberculous metamorphosis attacks cellular and transitory as well as fibrous and permanent elements.

"4. The tuberculous metamorphosis consists in a cessation of the nutritive and formative processes, in a mortification, death of the elements of the tissue, with subsequent peripheric absorption of the fluid constituents, and drying-up of the parts lying beyond the sphere of nutrition; the death itself of the elements of the tissue is caused by the accumulation of cell-elements, and is immediately determined by compression of the vessels of the part.

"5. These cells may arise from an absolutely new formation, or from an increased formation of the normal elements (epithelia, enchymkömer, &c.), or, finally, from an endogenous formation (ans einer endogenen Bildung).

"6. All these processes presuppose definite disturbances of the local nutrition, especially an altered exudation, and point back, accordingly, either to inflammation itself or to an analogous affection, no matter whether they owe their origin to an irritation produced by local mischief, or to an excitation consecutive to constitutional causes, primary changes of the blood, &c.

"7. There is therefore an inflammatory, cancerous, typhous, glanderous, sarcomatous, &c. tuberculization, which are altogether the same in reference to the essence of the local process, so far as this depends on tissue metamorphosis, but which are more or less different in reference to the essence of the whole process, as well so far as the latter is local (disturbance of nutrition, exudation, &c.), as also when it is due to general constitutional causes.

"8. Tuberculosis is the whole process of the affection, comprising the conditions of the local disturbances of the process of nutrition with the changes appertaining to it in the exudation, both in regard of the cell-formation and transformation, and finding in tuberculization its constant regular expression. Every tuberculization (tuberculous metamorphosis) does not have its origin in tuberculosis; tuberculosis can be present, as far as its early stages (exudation, cell-formation) are concerned, and yet there may be no tubercle. We shall therefore call that diseased process tuberculosis, which in its ordinary course always leads to tuberculization; while we shall ascribe cancer and sarcoma, which accidentally tubercularize, to an altogether different process, and shall never give the name of tubercle to a thickened abscess, pus become cheesy, pus concret.

"9. Scrofulosis is the constitutional affection which, after glanders and typhus, the most frequently produces tuberculosis—i. e., the local disease with the regular termination in tuberculization. But all its products are not tuberculous; tuberculosis is rather co-ordinate with a succession of other local processes.

"10. As tubercle is everywhere formed by the accumulation in the tissues of cells of the most varied kinds, these cells in the majority of cases breaking up, it has no peculiar characteristic elements. The shrivelled nuclei arising from the remains of the cells exhibit the greatest degree of constancy in their outward characters, and therefore we can retain for them the name of tubercle-corpuscles." (band ii. p. 72—74.)

What is Phthisis? A considerable portion of Virchow's papers is occupied by a discussion concerning the meaning that is and that ought to be attached to the term phthisis pulmonalis. He argues that there are various kinds of pulmonary phthisis, and that the practice of using the terms phthisis pulmonalis and tubercle as synonyms is founded in error.

Whenever cheesy-looking matter has been found in the lung, it has been considered, says Virchow, to be tubercular; while in reality, pus, cancer, &c., are equally susceptible of metamorphosis into this cheese-like substance. And whenever ulceration of the lungs has been found in conjunction with the presence of cheese-like matter, the case has been regarded as one of tubercular phthisis, when it may have been merely ulcerative bronchiectasis,

the cheesy-looking matter being only thickened pus. Virchow would have us adopt Morton's definition of phthisis.

"Phthisis pulmonalis est consumptio totius corporis cum febre, a mala affectione et ab ulceratione pulmonum tandem originem ducens. Quæ quidem est phthisis maxima famosa et καὶ ἐξοχὴν dicta, de qua autores tractare solent, tanquam nulla esset alia phthiseos species. Hæcce phthisis pulmonaris est vel originaria, quæ a mala diathesi et ulceratione pulmonum primo instante dependet, vel secundaria et symptomatica, quoties scilicet pulmones a morbis præcedentibus jam altius afficiuntur."

He would have us distinguish pulmonary tuberculosis from pulmonary phthisis, of which latter there are various kinds. Reinhardt has, Virchow considers, proved that a great part of the so-called tuberculous destruction of the lungs arises out of chronic suppurative pneumonic infiltration, and he adds, "the statements of Carswell, which are illustrated by such beautiful drawings, have experienced the more positive confirmation that Reinhardt worked without any knowledge of them." At the same time, Virchow maintains that the cheesy matter found by Carswell and Reinhardt in the bronchi and alveoli of the lungs did not deserve the name of tubercle.

The origin of cavities in the lungs is traced by Reinhardt, in many cases, to disease of the walls of the bronchi—viz., ulceration and abscess, leading to abscess in the pulmonary tissue, and subsequent gangrene of the cavity so formed. In some cases, the apparent cavities are dilated bronchi, the walls of which may become the seat of ulceration or of gangrene.

The propriety of distinguishing ulceration of the lungs, the result of the softening of tubercle, from destruction of the same organs from other pathological changes, is more generally admitted in this country than the statements of Virchow would lead us to suppose it to be in Germany. In illustration of our statement, we may refer to a series of valuable papers published some years since in the 'Guy's Hospital Reports,'* by Dr. T. Addison, in which he dwelt on the fact, that a large number of pulmonic excavations commonly considered to be the result of the softening of tubercle, are in reality due to pneumonia; and proposed to establish three varieties of phthisis—viz., Pneumonic Phthisis, Tuberculo-pneumonic Phthisis, and Tubercular Phthisis.

With reference to the first variety, he writes:

"This pneumonic phthisis may be acute; the deposits and inflamed tissues softening down and disorganizing at once, without any attempt whatever being made at induration or repair, thereby constituting one form of acute or galloping consumption.

"It may be *acuto-chronic;* of which I would distinguish three [two?] varieties:

"1. The inflammation, though more or less acute, is slower and more invidious in its course, and manifests some attempts at repair, as indicated by various stages and degrees of induration. The induration, nevertheless, is not complete; the pulmonary tissue continues to be friable; and sooner or later—that is to say, in a few weeks or months—softens down, and gives rise to excavations; most frequently by a slow ulcerative process; more rarely by an actual slough, of greater or less portions, of the indurated but still friable pulmonary tissue.

"2. Inflammation may supervene upon or around ancient induration, leading to disorganization either of the newly-inflamed tissue, of the old induration itself, or of both at the same time.

"Lastly, pneumonic phthisis may be chronic; of which I would also distinguish two varieties:

* Guy's Hospital Reports, 1837, 1843, 1845.

"1. That in which old indurations undergo a slow process of disintegration, giving rise to vomicæ.

"2. That very rare form of the disease, in which an insidious inflammation proceeds very slowly to convert a considerable portion of pulmonary tissue into gray induration without any excavation whatever."

In that form of phthisis which he terms tuberculo-pneumonic, Dr. Addison says, that "although tubercles are present, the really efficient cause of the phthisical mischief is pulmonic inflammation."

Some of the cases referred to by Dr. T. Addison were probably examples of bronchial abscess, so well described by Dr. W. Gairdner, in his able papers on the Pathological Anatomy of Bronchitis.*

Speaking of the lungs of a female, aged 30, who died of dysentery, Dr. Gairdner writes, they

"Presented great variations in density; the anterior edges were partially emphysematous, but between the portions thus affected could be felt numerous condensed parts, which, when superficial, presented a somewhat sunk, collapsed appearance, and a deep purple colour. At the posterior part of the lung were considerable masses similarly condensed. On cutting into the pulmonary tissue, there were seen throughout the condensed portions, numerous small yellow points, resembling softened tubercles, but more irregular in outline; these when scraped with the knife were found to be bronchial tubes, or small cavities, filled with and surrounded by pus. Except at these points, the condensed tissue yielded to the knife a little sero-sanguinolent fluid, which, when examined under the microscope, contained mostly blood-corpuscles, with a few-epithelium scales and pus-corpuscles."

Dr. Gairdner, in commenting on this case, observes, that it is the same affection of the lung as that figured by Dr. Addison in his third plate.

In the cases of pulmonary collapse of which Dr. Gairdner is speaking, the evacuation of the pus from the bronchial tubes is, as he remarks, prevented by the absence of *vis à tergo*. Now, when common exudation-matter, the result of chronic pneumonia, has been poured out into the substance of the lung, and obliteration of the air-cells has followed, there must be the same absence of *vis à tergo;* and consequently, inflammation of the bronchi in the consolidated tissue will be followed by the accumulation in them of their secretion, and ulceration of their walls; and finally by more or less rapid destruction, by ulceration and gangrene, of the consolidated pulmonary tissue.

While, then, we are inclined to regard some of Dr. Addison's cases as examples of bronchial abscess in collapsed tissue, we believe that in others the primary lesion was, as he himself thinks, chronic pneumonia. In Dr. Gairdner's cases, the order of the succession of the lesions was bronchitis, pulmonary collapse, bronchial abscess; in many of Dr. Addison's cases, at least exudation of lymph, obliteration of the air-cells, collection of purulent-looking fluid in one or more bronchial tubes, abscess.

Virchow very properly dwells on the importance of distinguishing the various kinds of phthisis from each other, when estimating the influence of hereditary predisposition, antagonisms, &c., on its occurrence.

In our next review we shall enter on the general consideration of Mr. Ancell's and Dr. Cotton's works.

W. Jenner.

* Edinburgh Monthly Journal of Medical Science, 1850.

PART SECOND.

Bibliographical Record.

ART. I.—*The Physical Diagnosis of Diseases of the Abdomen.* By EDWARD BALLARD, M.D. Lond., late Medical Tutor in University College, London, &c.—*London*, 1852. pp. 276.

ALTHOUGH in many systematic treatises, and in medical cyclopædias, chapters or articles are devoted to the physical methods of examining the abdominal organs, we are not aware that any special work has hitherto been published on this subject. This is not a little singular, since the physical signs of abdominal diseases may be said to have been studied at an earlier period, than those even of pulmonary and cardiac affections. They have not, however, attracted an equal degree of attention, and perhaps have not been cultivated with all the diligence they deserve. Dr. Ballard's work has undoubtedly met a want which all have more or less felt; and we have no hesitation in saying, that the manner in which the want has been supplied is deserving of high commendation.

The work is divided into three Parts: the First treats of the methods of examining the abdomen and its organs, by inspection, mensuration, palpation, percussion, and auscultation. On this part we have little to say. It is well done, and, as far as we can see, no important fact has been omitted. The various methods are detailed in clear language, and the subdivisions of each chapter have been evidently arranged with no little care and attention.

In the Second Part, the diseases of the abdomen are considered *seriatim*, and the physical signs of each are enumerated. Two or three extracts will serve to give an idea of the manner in which this division of the work has been executed.

"The physical signs of *simple congestion of the liver* are those of enlargement (86 *et seq.*) in a degree commensurate with its amount. The liver may thus appear to have undergone very little increase in bulk, or, on the other hand, it may be greatly enlarged, especially in cases of cardiac disease, where it is often conjoined with some textural alteration and hypertrophy (95); it may, in the latter case, reach as low as the level of the spine of the ilium. In the pure form of congestion, it is rarely that *palpation* can discover the margin of the organ so as to ascertain its characters, partly on account of the fulness of the colon with gas and accumulated fæcal matter, and partly because there is no laxity of the abdominal wall, while the tenderness may in some cases occasion an involuntary muscular resistance to the pressure of the hand. All that palpation can mostly discover, is deficient yielding to pressure beneath the margin of the ribs on the right side, when compared with the left, to the extent of a few fingers' breadth. When the edge of the liver can be felt, and the character of the enlarged organ ascertained, the former will be perceived to be even, and only slightly thickened, and the latter to

be smooth. *Percussion* is more useful in determining the dimensions of the organ. The increase of dulness is generally in all directions, but it may be perceived to occur principally in the downward direction, or it may occur principally upwards, so as to encroach upon the pulmonary resonance, while but little is perceived below the margin of the ribs. The dulness does not shade off into the resonance of the intestines quite so perfectly as it does in health. An important and interesting character in enlargement from congestion, lies in the rapidity with which its indications sometimes subside within the course of a few hours." (p. 71.).

As another example of this part of the work, we select the following account of the physical signs of cancer of the pylorus.

"Cancer of the pylorus is, for the most part, not indicated by any *visible* external sign, but when involving neighbouring tissues, as the pancreas and mesocolon, it may produce visible elevation and prominence over the epigastric or upper umbilical regions, which may extend even to the pubic symphysis, and be accompanied by visible pulsation. Palpation commonly discovers superficial or deep-seated tumour; but in the early stage of the disease it requires a careful search to be made for it at different times of the day, and after recent and full evacuation of the bowels; there may, however, be considerable tumour present, and yet it will escape the hand, from being overlapped by the liver, or obscured by various conjoined tumours within the abdomen. Its usual seat is a little to the right of the median line, and from one to three inches below the margin of the ribs. As the disease advances, and the tumour increases in size and weight, it gravitates more or less towards the lower regions of the abdomen, and may be felt in situations where it might be little expected to be found, such as the umbilical or right iliac regions, the right flank, below the cartilages of the left false ribs, or over the pubes. Its size varies from simple palpable thickening to that of an egg or larger, and its surface may be either smooth or nodulated. Its consistence is mostly hard, but sometimes it presents a certain amount of elasticity. It is commonly moveable, being found to alter its position a little, according as the stomach is full or empty, descending towards the navel if the patient sits up or stands, and during the acts of inspiration; and moving a little towards either hypochondrium when he turns upon the corresponding side. As in the case of any other tumour, however, there may be such adhesions to the parietes and surrounding organs as to render it completely immovable; and when the parts behind are involved in the disease, the tumour may be firmly fixed to the spine. It sometimes pulsates, and the pulsation may be most remarkable when the stomach is full. In any case of cancer of the stomach, the palpable signs of tumour may be obscured by muscular resistance to the pressure of the hand. *Percussion* over the tumour elicits a modified dulness. Occasionally there has been *heard* over the tumour a murmur transmitted from the aorta behind it." (p. 111.)

These extracts are sufficient to show the kind of information contained in this Second Part, and the manner in which it is given. It appears to us both full and accurate.—The Third Part is the most original portion of the volume, and must have cost its author no little labour. The various physical signs are enumerated, and under the head of each, all the pathological conditions which may cause it are arranged. Thus, if a physical sign is once recognised, we have, at a glance, all its known causes, and can thus judge from other circumstances which cause may be present in the particular case under consideration. We cannot conceive anything more useful for a student than a thorough training in this method, and even an experienced practitioner would find it very useful to refresh his memory occasionally with a glance at this part of the work. The following extract will exemplify our meaning. Under the head of *inspection* are arrayed, of course, all the enlargements of the abdomen, or of any part of it. En-

largement in the right iliac region may be caused by no less than twenty-three different conditions—viz.

1. Flatulent distension of cæcum (152, 153).
2. Fæcal accumulation (154).
3. Intestinal concretions (155).
4. Intestinal obstruction, above cæcum (156).
5. Typhlitis (159).
6. Cancer of cæcum—extensive (163).
7. Circumscribed peritoneal abscess (175).
8. Cancer of peritoneum (180).
9. Displacement of bladder upwards and to right.
10. Pelvic inflammation and abscess (187).
11. Pregnancy with lateral obliquity (203).
12. Extra-uterine pregnancy (209-213).
13. Retention of menses (214).
14. Fibrous tumour of uterus (224-220).
15. Inflammation and abscess of ovary (230).
16. Encysted disease of ovary (231).
17. Solid ovarian tumour (247).
18. Aneurism of aorta (249).
19. Aneurism of iliac artery (256).
20. Phlegmon and abscess in wall (260).
21. Œdema of wall (5).
22. Tumour in wall (261, 262).
23. Psoas abscess (263).

The numbers at the end of each line refer to the paragraphs in which the other physical signs of the particular condition are given, so that by reference to these paragraphs, we may, by the aid of the other signs, determine which of the twenty-three pathological states is present.

A very copious index is given at the end of the work.

In concluding this short notice of Dr. Ballard's new work, we have only to say that it is a production rather to be studied than to be reviewed; and we are convinced that the more it is studied, the more its numerous facts will be appreciated.

ART. II.—*Proceedings of the Pathological Society of London. Sixth Session.* 1851-52.—*London,* 8vo, pp. 490.

THE London Pathological Society pursues its investigations with zeal, and, as the present volume proves, with success. We think it, indeed, the best volume which has yet been issued. On comparing the descriptions of the preparations given in this, and in former volumes, it is impossible not to perceive that the majority of them are better done than heretofore. The enumeration of the coarser physical characters is given with greater care, and the microscopic appearances are detailed with infinitely more fulness and correctness. The Society has in fact educated itself, and as it is still in the vigour of youth and growth, we do not doubt that its improvement will continue.

In reading over this and the former volumes of Proceedings, the question

has forcibly presented itself to us, whether the Society might not now attempt something more ambitious. Are these numerous facts to bear no further fruit than what may be drawn from them here and there by some hard-working student? Could they not be brought to bear on each other, be compared and analyzed, so as to allow the deduction of some general expression? Medicine at the present day is like a heap of stones; every one brings his pebble;—but where is the master hand to build them up in order?

Again, we would say, could not the Society push farther its principle of combination? In the present volume, they have carried to some length the practice of appointing one or more of their members to make microscopic examinations of specimens exhibited by other members. The results are most interesting, and prove what power combined working has in this case as in all others. But could not the Society have a more efficient combination than this, which, after all, is the rudest form of it? Could not some special class of diseases be investigated, or some particularly obscure points in pathology be systematically worked out? Some two or three hundred able and practical men, all labouring for one object, would surely achieve some great results.

In order, however, to do this, the Pathological Society must work upon a system, and the first labour is to settle what that system is to be. And here we will make our last suggestion, and at the same time entreat the Society to believe that we are actuated by the most friendly motives in doing so. Many of the cases in their Proceedings are most ably drawn up, but others are not so. It is evident that either in the original they have been imperfect, or that in the abstract they have suffered damage. Condensation may be carried too far, and it is a great mistake to save space at the expense of completeness. Sometimes a good description is given of a single valve in the heart, all the other valves, and the condition of the cavities, being left unnoticed. Attention is concentrated upon a single point, and all coincident conditions are disregarded. This is surely not the way in which pathology is to be cultivated. No doubt if a man takes a heart from one body, and a lung from another, he may arrive at curious results, but he will never develop the utmost consequences from his facts. In every case, were it possible, we ought to extend our researches beyond the narrow sphere of a single organ. Could the Society not devise a method of observation to be used by all its members, which might aim at some more regular and complete record of pathological states? The members of the Pathological Society have learning, industry, and zeal. If they would only work together more systematically, their exertions might create a new era in medicine.

ART. III.—*A Complete Treatise on Midwifery, &c. &c.* By ALF. A. L. M. VELPEAU, M.D. Translated by Professor MEIGS, M.D., Philadelphia. Fourth American Edition. Edited from the last French Edition by W. B. PAGE, M.D.—*Philadelphia*, pp. 652.

IT is much to the credit of our American brethren that they have preceded us in the translation of many standard French works on midwifery. To Professor Meigs we are indebted for an excellent translation of Velpeau,

which has reached a fourth edition. One of the admirable points in Velpeau's original work is the extensive research which is rendered available to us by his minute and upon the whole accurate references. We are sorry to find that in the American translation these latter have been omitted. Their insertion would, doubtless, have increased the expense, but the advantage, we conceive, would have been more than a counterbalance. It is scarcely necessary to say, that the translation is well and faithfully executed; the names of Dr. Meigs and Dr. Page are a sufficient guarantee for that. The work is, however, little more than a translation; there are a few foot-notes, on practical matters, by the editors, but not so many as to give a compound character to the work. One of these notes we shall extract, as expressing Dr. Meigs' opinion upon two points in which he differs from M. Velpeau. The latter has been enumerating the sources of danger to the child in pelvic presentations—such as pressure upon the abdomen and chest impeding the circulation, and determining blood to the head; upon which Dr. Meigs remarks, very justly—

"The view taken here of the causes of death which so frequently operate effectually on the fœtus in pelvic presentations, omits one of the most considerable, and which appears to me to be readily conceived of in the following manner. When the vertex descends first, in any woman, the child begins to breathe as soon as the mouth and nostrils are exposed to the air, and it generally cries before the shoulders are born; but when it is enabled to reach the air, it becomes instantly a matter of indifference, as to its security, whether the afterbirth be detached or not. Now it most generally happens, that the afterbirth is wholly or partially detached by the contraction of the womb, long before the hips and legs of the child are expelled; for the womb is by this time grown so small that the placental superficies of it can no longer hold the placenta. This, as I have said above, is a matter of indifference to the child as soon as it can communicate with the atmosphere. In a pelvic presentation, on the contrary, it is a matter of the greatest consequence to the child's safety, that the detachment of the placenta should not take place so early; for although the feet or the breech are born, the child's head having no access to the air, it perishes with real, I might say with double, asphyxia—to wit, the placenta is separated from the mother, and its lungs receive no air. I am far from asserting that the placenta is detached in all cases at so early a stage of labour, as that which I have indicated, though I am free to utter my opinion, that, in the vast majority of cases, the placenta is separated by the time the head is fairly born, in ordinary vertex cases."

Dr. Meigs says, that the results in his own practice are more favourable than those given by M. Velpeau, which he attributes to his custom of sending for his forceps so as to have them at hand whenever he detects a pelvic presentation. "I make very slight traction on the shoulders, in order to facilitate the expulsion of the head; and as soon as I find that the head is not likely to come down, I grasp it in the forceps and deliver it at once. I have safely delivered a number of children which I think would have been born dead but for such a precaution."

It is right to state, that this translation is virtually from the last French edition, which, as our readers may be aware, has been very much enlarged, and in many respects rearranged by the author. The merits and defects of M. Velpeau's work are probably as well known to our readers as to ourselves, and we are quite sure that they will cordially welcome his appearance in his English dress.

ART. IV.—*Quarterly Journal of Microscopical Science, including the Transactions of the Microscopical Society of London.* Edited by EDWIN LANKASTER, M.D., F.R.S., and GEORGE BUSK, F.R.C.S.E., F.R.S. No. I. Second Edition.—*London*, 8vo, pp. 64.

WE congratulate all lovers of natural history on the issue of this journal. We are happy to observe that the first edition has been entirely sold off, and we have little doubt that its future issues will be equally successful. The journal is divided into two parts; the first being the Transactions of the Microscopical Society, and containing papers on Lacinularia Socialis, by Mr. Huxley; on the Raphides of a Cactus, by Mr. Quekett; on a cyst upon an olfactory nerve of a horse, by Mr. Simonds; and on the development of Tubularia Indivisa, by Mr. Mummery. The second part, which is separately paged, is the journal proper, and is occupied by papers on the anatomy of Melicerta ringer, by Mr. Williamson; on the contractile tissue of the Iris, by Mr. Lister (*vide* Chronicle of Medical Science); hints for collecting objects for microscopical examination, by Mr. Shadbolt; and on the cellulose in the tunic of Ascidians, by Mr. Huxley. Then follow translations of Kölliker's paper on Actinophrys Sol, and Schacht's observations on the mantle of some Ascidians. Reviews and news fill up the remaining space.

We have given the list of the papers, to show our readers what kind of fare they will find in this journal.

The getting-up is extremely good, and both editors and publisher seem to have done their utmost to do justice to their theme.

ART. V.—*The Fever at Boa Vista in 1845-46, unconnected with the visit of the 'Eclair' to that Island.* By GILBERT KING, M.D., R.N., Inspector of Hospitals and Fleets.—*London*, 1852. pp. 110.

DR. KING was sent out to Boa Vista in 1846, after Dr. M'William's return thence, and he presented a Report on the fever which had been prevalent there, which was reviewed in this journal.* This report was replied to by Dr. M'William, and was severely criticised by Sir William Pym; and the present publication is a reply to the observations of these two gentlemen. We have lately gone into this controversy at such length, that we must be excused from again considering it. We have looked through Dr. King's Report, but without finding that he has brought forward any new facts. He has made a minute criticism of Dr. M'William's evidence, which any one is competent to do for himself. We may remark, however, that there is one point in which Dr. King is a little disingenuous. Speaking of the evidence contained in Dr. M' William's Report, he writes—

"In the discussion of this question, the arguments in support of contagion are founded solely on the assumption, that Anna Gallinhá died in Port Sal Rey on the 16th of October, but of that fact we have no adequate proof, and in default of any authentic particulars of the outbreak and progress of the epidemic, we are required to substitute the oral testimony of ignorant and illiterate persons in the very lowest grade of civilized society, upon whose carefulness in observing, accuracy in remembering, and truthfulness in narrating, the exact dates, and the precise order in which the events are said to have happened, the whole case rests." (p 21.)

* Vol x p. 285.

No one will dispute the justice of these opinions; but it so happens that the evidence concerning the death of Anna Gallinha was given, not by any of these ignorant, illiterate, and uncivilized persons, but by an Englishman, John Jamieson, the consul's storekeeper.

Dr. King himself afterwards says "The only authority for the dates of the attack and death of Anna Gallinha is the oral testimony of John Jamieson." (p. 30.)

Having thus ingeniously argued that the date of this woman's illness *cannot* be known, because the testimony is supposed to be of a particular kind, it would be just as fair to affirm that the date *can* be known, because the testimony is proved not to be that which has been condemned as inaccurate. We must say, this single point lessens our faith in Dr. King's impartiality.

ART. VI.—1. *A Manual of Elementary Chemistry.* By GEORGE FOWNES, F.R.S. Fourth Edition.—*London.* 8vo, pp. 681.

2. *A Practical Handbook of Medical Chemistry.* By JOHN E. BOWMAN, F.C.S.—*London.* 8vo, pp. 261.

THESE are two standard works, the re-issue of which we need scarcely do more than announce. Mr. Fownes' Manual preserves its reputation of much knowledge in little space; and Mr. Bowman's second edition possesses the same exact and careful division of subjects as his first. We could certainly have wished that the portion devoted to animal chemistry in Mr Fownes' work had been longer. Thirty-three pages out of 681 is surely an inadequate amount; and at a time when the unexampled work of Lehmann has appeared, the standard by which the matter of this part must be judged, is high. There are several points in Mr. Bowman's generally very accurate work to which we must also take some exception. He still inserts a table for calculating the solids of the urine from the specific gravity, although nothing is more fallacious than this method, which is not even accurate enough for the roughest calculation. The use of the urinometer is only to detect extremes. Again, in determining the solids of the urine by evaporation, 1000 grains are directed to be taken, a quantity far too large to be thoroughly dried except with immense trouble. We observe, also, that the common sediment of the amorphous urate is said to consist chiefly of urate of ammonia. We had thought it now admitted that the so-called urate of ammonia is for the most part urate of soda, with some mixture of urate of lime and of ammonia.

The usual precision of the book makes us more anxious to direct attention to these few inaccuracies, which do not in the least diminish its general value.

ART. VII.—*The Dictionary of Domestic Medicine and Household Surgery.* By SPENCER THOMSON, M.D.—*London*, 1852. Small 8vo, pp. 572.

NEARLY 600 pages of close print are here published for the sum of six shillings. When we add that the paper is good, the type legible, and the matter of the work useful and practical, we shall have said enough to convince every one that this is one of the cheapest publications we know

The subjects are arranged alphabetically, and frequent reference is made from one topic to others, so that without much trouble any one may acquire a good deal of information on any medical or hygienic subject. We are bound to say that Dr. Thomson has executed a difficult and delicate task with success, and has succeeded in producing a book which will give the *laity* sound information on many points, which it is for the interest both of themselves and of the profession to know. No rule is more pernicious than that of "every man his own doctor;" but nothing is more useful to a medical man than to have an intelligent patient. Dr. Thomson's book will have the effect of making its readers not only know more of medical science, but trust more in medical men. Ignorance is the parent of quackery—that is, of faith misplaced; and there is no better antidote to quackery than a correct knowledge, even though this may be limited. In addition to this, Dr. Thomson's work will undoubtedly be found useful both in the sick room and in families remote from towns, and medical men; and it will probably supersede the old treatises on domestic medicine, which, bad in the beginning, are now a disgrace to science.

ART. VIII.—*Demonstrations of Anatomy.* By G. VINER ELLIS, Professor of Anatomy in University College.—*London*, 1852. pp. 822.

WE need not do more than announce the appearance of the third edition of this excellent text-book and dissecting guide. Some few alterations have been made, but the bulk of the volume remains the same as in the second edition.

ART. IX.—*Handbuch der Gewehlehre der Menschen, für Aerzte und Studirende.* Von A. KÖLLIKER.—*Leipzig*, 1852. pp. 637.

THIS is an abstract of Kölliker's larger work on 'Microscopic Anatomy.' In many places the text is altogether the same, and after a glance through it, we do not perceive that it contains any novelties. It will no doubt prove to be very useful for students and practitioners, as in a comparatively short space it gives a summary of our knowledge up to the present time.

ART. X.—*Specielle Pathologie und Therapie.* Von Dr. E. A. LUDWIG HÜBENER.

Special Pathology and Therapeutics. By Dr. HÜBENER.—*Erlangen.* pp. 538.

THIS is the second and final volume of a work, the first part of which was issued in 1851. Although only intended to be a kind of abstract of Canstatt's and Wunderlich's great works, it is itself of no inconsiderable length. It is a useful and sufficiently exact, though not a first-class work. The most remarkable feature in the present volume is the space devoted to the subject of poisons. The chronic diseases produced by mercury, zinc, silver, copper, lead, phosphorus, aconite, strychnine, and in fact by some eighty mineral or vegetable substances, when introduced into the body, accidentally or medicinally, in too large doses, are detailed at length. This is a subject which in English works has been much neglected, and has been considered to be too strictly in the province of the toxicologist.

Art. XI.—1. *A Letter to Dr. Lyon Playfair on the recent Analysis of the Buxton Tepid Water.* By W. H. Robertson, M.D.—*London*, 1852. pp. 19.

2. *A Guide to the Use of the Buxton Water.* By W. H. Robertson, M.D. Seventh Edition.—*London*, 1852. Small 8vo, pp. 40.

The following analysis of the Buxton tepid water has been lately made by Dr. Lyon Playfair.

IN ONE IMPERIAL GALLON.

	Grains.
Silica	0·666
Oxide of iron and alumina	0·240
Carbonate of lime	7·773
Sulphate of lime	2·323
Carbonate of magnesia	4·543
Chloride of sodium	2·420
Chloride of potassium	2·500
Fluorine (or fluoride of calcium)	a trace
Phosphoric acid (or phosphate of lime)	a trace
	20 579

In addition, the water contained some carbonic acid, and an extraordinary amount of free *nitrogen*, amounting to no less than 206 cubic inches in an imperial gallon.

Dr. Robertson believes that the discovery of this large proportion of nitrogen will explain the undoubted good effects of the Buxton water. Its first effect is strongly stimulating, and its second is depressing. It therefore requires some caution in its use. It is chiefly useful in chronic rheumatism, gout, sciatica, dyspepsia, general feebleness, and some cases of paralysis. It is also often very useful in catamenial irregularity.

The chalybeate Buxton spring contains, according to Dr. Playfair's analysis, about one grain of proto-carbonate of iron in the imperial gallon.

Art. XII.—*Principles of Human Physiology.* By William B. Carpenter, M.D., F.R.S., &c. Fourth Edition.—*London*, 1853. pp. 1140.

This new edition of a justly esteemed work has been somewhat enlarged and greatly improved. Many parts have been entirely re-written, and the latest discoveries of physiology and of physiological chemistry have been carefully included. Among the new portions which have most struck us, is the second chapter, "On the chemical components of the body, and the changes which they undergo within it." The chief facts detailed in Lehmann's great work are included, and are narrated in Dr. Carpenter's clear and easy style. Physiology is advancing in this department with extraordinary rapidity, and it is evident that it must soon react upon practical medicine. The chapter on the blood is also of great interest. In the anatomical part of the work, the most recent researches of Kölliker and other authorities are given; and in the subsequent chapters on food and digestion, sanguification, circulation, and nutrition, every recent observation has been incorporated. The chapters on the nervous system have, however, undergone the greatest alteration, and this large portion of the

work may be said to be almost altogether new. It is in this direction, we think, that Dr. Carpenter's chief strength lies, and we know no one who is equally able to deal with this vast and almost illimitable subject. In dealing with it, he appears more in the character of an original thinker, than in any other portion of the work, although everywhere his powers of combination and acuteness in deduction, enable him to educe original conclusions from the facts collected by others, or gathered by himself.

It would be useless to review this work by itself, but its publication will afford us an opportunity of considering the various topics of which it treats, and of applying, as far as can be done, the discoveries of physiology to practical medicine. In the mean time we commend it to our readers, as the most complete exposition of physiology which any language can at present give them.

We must not omit to mention that many of the woodcuts and plates appear to be either new or re-cast. The whole work is copiously illustrated.

ART. XIII.—*Pulmonary Consumption and its Treatment.* By W. M. BURSLEM, M.D.—*London*, 1852. pp. 160.

THIS work would have been improved, if the whole of the first part had been omitted. It contains some useful facts on the subject of the treatment of Consumption, but the opening chapters on the anatomy and symptoms of Phthisis are little more than a meagre statement of the doctrines of some of the better-known writers. The reader has consequently to wade through some fifty pages of unnecessary matter before he arrives at the really useful part of the treatise. Dr. Burslem has been led to form a high opinion of the use of emetics in phthisis, especially in the early stages; he has given them an extensive trial, and has also collected together the not inconsiderable evidence in their favour which may be found in the annals of medicine. He prefers the essence of ipecacuanha, (prepared for him by Twinberrow, of Edward-street, Portman-square) as it operates in ten or fifteen minutes, and leaves no depression behind it. He gives it every third or fourth day, or every week, and usually in the morning. He states, that it dislodges the sputa, and diminishes the dyspnœa and cough. With emetics, Dr. Burslem combines cod-liver oil and nourishing diet, and sulphuric or gallic acid, when there is any tendency to hæmoptysis. Twenty cases are detailed as proof of the utility of emetics, but the treatment appears to have been usually so complicated that it is difficult to draw any very decided conclusions. Looking, however, to the experience of other observers, and to some of the most marked instances recorded by Dr. Burslem, we cannot but believe that emetics in the early stage of phthisis have been occasionally very beneficial, and that their employment has been too much neglected. Dr. Burslem has done good service by again calling attention to them.

PART THIRD.

Original Communications.

ART. I.

On the Employment of Iodide of Potassium as a Remedy for the Affections caused by Lead and Mercury. By M. MELSENS. Translated from the paper in the 'Annales de Chimie et de Physique,' June, 1849, by WILLIAM BUDD, M.D., Physician to the Bristol Royal Infirmary.

PREFACE.

THE readers of the 'British and Foreign Medico-Chirurgical Review' will probably require no apology from us for presenting them with the following translation of M. Melsens' Memoir on the Treatment of Metallic Poisoning by Iodide of Potassium.

The great practical importance of the facts it sets forth, and the highly interesting character of the evidence by which they are authenticated, give it a double claim to the serious attention of all who practise the healing art.

If, as M. Melsens alleges, and as his numerous experiments seem conclusively to show, iodide of potassium is not only a safe, certain, and radical cure for the common forms of saturnine and mercurial poisoning, but an equally sure preventive of the injurious effects so frequently produced by emanations from lead and mercury, the fact is one which cannot be made too widely known.

When we consider the great number of working men who, from the nature of their occupation, are constantly exposed to the poisonous influence of these deadly metals, such a result would of itself deserve to rank as one of the most important discoveries of the day. But M. Melsens' researches have a yet wider scope

The relation which he seems to have established as occurring between iodide of potassium and mercury when present together in the living body, is not limited to the case of mercurial poisoning, but has an important bearing on the therapeutic use of these agents, more especially in the numerous cases in which, according to present practice, they are given concurrently, or in near succession. This can only be fully understood by those who have read the Memoir itself. Some notion of it may, however, be formed by considering for a moment the mode in which M. Melsens supposes the iodide of potassium to exert its curative power.

In all cases of mercurial and saturnine poisoning, he assumes, and no doubt rightly, that the metallic substance is in actual union with the

affected part or parts, and is retained there in the form of some *insoluble* compound.*

According to his view, the iodide of potassium, after its absorption into the blood, combines with the metallic poison, and forms with it a new and *soluble* salt—liberates the poison from its union with the injured part—dissolves it out, so to speak, from the damaged fibre, and sets it once more afloat in the circulation.

The new compound thus set at liberty (under the form, it is presumed, of a double iodide of mercury and potassium) he supposes to be eliminated through the kidney almost as soon as formed, in combination with any excess of iodide of potassium that may happen to be present. So that, both poison and remedy being cast out together, the cure may be said in a peculiar sense to be radical and complete.

Surely this is a mode of action which, quite independently of the practical importance of its application in the present instance, is one of the very highest order of interest; and not the less, because there is much reason to believe that it may be the type of what occurs in many other cases.

I do not know whether M. Melsens' researches have been verified or not by other observers. It would certainly have been more satisfactory if results so novel and striking as are here laid before us had been confirmed by other and competent hands. It is right, at the same time, to say that the evidence on which he rests his case, although not in some points so complete as might be desired, is on the whole sufficiently conclusive. With regard to the case of mercurial poisoning it is especially so. Many instances are given of the rapid cure of this terrible affection by iodide of potassium, including one in which there was chemical proof of the elimination of mercury through the kidney during the treatment. The documents showing the progressive and daily improvement, in the handwriting of the same patient, while taking the iodide, may also be referred to as possessing a peculiar interest as a piece of medical evidence.

Although I have had no opportunity of putting M. Melsens' doctrines to the test, *chemically*, since his Memoir first fell into my hands, it may not be out of place to mention, that in regard to mercury many facts had already come under my observation, which, so far as clinical evidence goes, seem to offer a very striking confirmation of them; so striking, indeed, that long before seeing M. Melsens' paper, I had been led by the force of evidence to anticipate the author's theory, in the interpretation of the particular cases to which I refer.

The first case was that of a man who was admitted to the Bristol Royal Infirmary about three years ago, for the cure of secondary syphilis, affecting his throat, joints, and bones. He was also the subject of partial paralysis of the lower extremities, which had come on concurrently with the other symptoms. He had had chancre some months (as well as I remember, about five months) before, for which, from what I could learn, he took mercury largely. From that time, however, up to the day of admission to the Infirmary, he had not taken any mercurial compound. The day

* Conclusive evidence might be given of the truth of this assumption, if it were needed. I may be pardoned, perhaps, for referring to some considerations bearing upon it, which were advanced by myself in 1841, in a paper on the Symmetry of Disease, published in the 25th vol. of the Medico-Chirurgical Transactions, although these considerations by no means exhaust all that might be said on the subject to the same effect.

after he came in he was put under treatment by iodide of potassium in pretty free doses. My surprise was great on coming to the hospital a few days afterwards to find my patient profusely salivated. The swollen face, the peculiar state of the tongue, the loosened teeth, the ulcerated mouth, and the characteristic fœtor, were all present. For many years I had not seen a severer case of mercurial ptyalism. And yet this man, as I specially ascertained—for I at first thought there might have been some mistake—had not taken a grain of mercury since his admission.

Under these circumstances, I came to the conclusion that a portion of the mercury administered some months before had become fixed in the body, and that the liberation of this mercury under the solvent influence of the iodide of potassium was the cause of the severe salivation under which the patient was now labouring. No other theory of the case, in fact, seemed admissible. It may be interesting to mention, that under a continuance of the iodide, not only the syphilitic affection soon got better, but the paralysis also was almost entirely removed.

Since the occurrence of this case, I have seen many others of exactly the same kind, in which mercurial ptyalism came on during the use of iodide of potassium, in persons who had taken mercury some weeks or months before, but none within a more recent period. On several occasions, indeed, the facts have been so striking, that I have been led to make them the subject of clinical remark. A very interesting example of it is under my eye at the present time, in which the same result, in a milder degree, has appeared to be produced by the free innunction of compound iodine ointment. The lady who is the subject of the ptyalism, took a mild course of mercury about ten weeks ago, but was not then sensibly affected by it.

On the subject of lead poisoning I have at present but little to add. A very interesting case of lead palsy is now under my care, in which I have adopted M. Melsens' plan, and apparently with great success. As the case is, however, still in progress, and many important details in it have yet to be filled up, it would be premature to speak of it more at length.

In conclusion, it is hoped that M. Melsens' Memoir, by the wider publicity now given to it among English practitioners, may, by the interest and importance of the results it sets forth, stimulate others to undertake similar inquiries in other branches of therapeutics.

Most of us must have felt, at one time or another, that the state of our knowledge respecting the action of medicines is the least satisfactory part of our art. Much of this depends, no doubt, on the great difficulties which beset the subject, but not a little arises also from the want of searching investigations bearing directly upon it. The position we hold in relation to the whole subject is, in many respects, a reproach to us. Seeking at every hour to modify the phenomena of life by the introduction of common chemical agents—often in large amount—into the living economy, nothing can appear more strange to a philosophic inquirer than the almost entire indifference we show as to what becomes of these agents after their absorption. Whether they abide in the living body, or merely pass rapidly through it; whether, in exerting their power, they are resolved into their elements, or retain their original form; in what manner, and in what company, or through what outlets, they are finally cast forth, with many other questions of equal interest, are secrets, into which, for the most part,

we care not to inquire. Yet there can be no doubt, that not one of these questions can be indifferent to a right comprehension of the wonderful and complex part these agents play in the cure of disease. If we were never beset by doubt or difficulty in their use, the case would be different. The fact that, on the whole, we have learned to use them with success, and even, perhaps, with most success where our knowledge of their power is most empirical, is no valid excuse for not endeavouring to throw light on their action by all the means we have.

These researches of M. Melsens, and others which could be referred to, show what invaluable results may be obtained by subjecting such questions as those just now cited to a well-devised system of chemical research. There are many problems in therapeutics, which, it is scarcely necessary to say, cannot be solved by any other means. How many of the obscurities and uncertainties which beset the action of mercurial compounds, for instance, and which so often baffle the physician in their employment, can only be cleared up by a well-directed series of chemical investigations.

To ascertain, if nothing more, the exact distribution of the drug in the several structures of the body, in fatal cases following its employment, and, in particular, the degree in which the diseased part or parts have shared or not in its appropriation, could not fail to furnish results of the highest interest. Subjects of similar inquiry invite us on every hand. And yet, with the exception of some recent researches by Dr. Bence Jones (which may be quoted as models of their kind), this Memoir by M. Melsens, some contributions by Magendie, Liebig, and others, and some important results incidentally obtained by the toxicologists, little or nothing of a serious kind has yet been attempted in this line of investigation.

It should be always remembered, that the subjects with which we are dealing are not subjects of mere scientific curiosity. They are not merely abstract speculations, but problems which we ourselves are daily working out to their practical ends, in the terrible issues of life and death.

To place our knowledge of the action of medicines, as far as it is susceptible of being so placed, on the sure footing of scientific truth, is clearly our most pressing want. Not to speak of the infinitely higher interests concerned, much of the discomfort that too often attends our relations with the public may be ultimately traced to the comparative uncertainty which besets the use of a large number of remedial agents. To reduce this uncertainty to its lowest possible point, no effort should be spared, no mode of inquiry should be left untried. Our duty to the sick, our own conscience, loyalty to our profession, alike require this of us. Doubtless, in this matter, as in most others, or rather, one would say, more especially in this matter than in most others, the elements of truth are not to be found in any single order of facts.

But in appealing to chemistry to help in their detection, we are not only encouraged by fruits already gathered in the same field, but following a path plainly indicated by the course of discovery in physiology itself. For to chemistry we certainly owe the discovery of the highest truth yet realized in this branch of human knowledge. What would be the condition of physiology now, what should we know of the phenomena of life, and of the great cardinal relations of living forces, if Lavoisier had never been, and if the nature of respiration were still unknown to us? Even the use of drugs

in the treatment of disease would have remained without its highest sanction. In the incessant dependence of all the phenomena of life, including the most transcendant, on the chemical action of a substance external to us (oxygen), we have not only a warrant coeval with the creation of animal life itself, for seeking to modify these phenomena by the employment of common chemical agents, but a living and enduring proof that even in their most subtle dynamic effects, these agents are inexorably governed by quantitative relations as strict as those which regulate their grosser actions—relations of which weight and measure are the only exponents.*

William Budd.

MEMOIR.

Medical men will, no doubt, pardon me for having, in this memoir, treated the question of the cure of the disorders produced by metallic compounds in a purely chemical point of view. I leave it to them to discuss the effects of iodide of potassium, considered with reference to the various cases and symptoms which these disorders present, as also to determine the influence of the secondary methods of treatment, by which, without doubt, this mode of cure may be rendered more rapid and energetic.

From the very outset of my researches, I have always looked at the subject in its simplest aspect. I have never, in fact, kept in view more than two definite things: *the disease from the presence of poison in the system, and the cure by the expulsion of this poison out of the system.*

It is difficult to form an idea of the inherent obstacles which stand in the way of investigations of this kind; obstacles which, indeed, present themselves in every attempt to connect together, in the same individual case, medical observations and chemical or physiological researches. It is my intention to recur at some future period to the anatomical changes, and especially to the physiological effects of metallic poisoning, which I have not, as yet, been able to clear up in a satisfactory manner. Thus, when I have endeavoured to trace the road which the different iodides follow in the body, all that I have been able to establish is, that they must differ strikingly in this respect; but the experiments I have hitherto made have not enabled me finally to solve such a delicate question. It appears to result from many experiments, that the iodide of potassium does not tend indifferently to every part of the body, but that marked differences occur in the quantity found in different organs. Thus, blood from the heart and blood from the liver do not appear to contain similar quantities of the iodide; differences appreciable to the eye are discernible not only between the heart and the liver, but also between the lungs, the spleen, the kidneys, the brain, the eye, the tongue; iodides are found in the liver, when they are absolutely wanting in the liquid of the gall-bladder; the mucus (serum ?) which bathes the intestinal canal, the pleura,

* It is proper to add, that in the following translation I have, for the sake of brevity, omitted the first page of the Memoir, which contains no information on the subject of which it treats, and consists merely of prefatory matter. It appears that these researches were begun by M. Melsens, in conjunction with M. Guillot, in 1843, in the private laboratory, and under the auspices, of M. Dumas. MM. Melsens and Guillot continued to work together for some time, but were at length separated by circumstances. Some more decisive experiments made by M. Melsens, since he has been parted from his friend, have led to the publication of the whole series of researches under his name. I have also omitted the details of some experiments in §§ 10, 11, 12, 13, and 18, and also the last paragraph of the Memoir.

&c., contains compounds of iodine, but the matters found in the intestine itself contain ordinarily none beyond the first half of the gut.

Iodide of potassium is found in the intestinal canal much below the *ductus choledochus*, when the bile contains no trace of iodine; but if, according to the notions generally entertained on the localization of poisons, the poisonous double compounds of iodine pass in part through the gall-bladder, in order to be cast out into the intestine, then they must of necessity be taken up again into the system, since in the neighbourhood of the *anus* no iodides are ever found. This well-established fact would explain why, independently of other circumstances, the cure is slow, and the intervention of secondary modes of treatment might possibly be useful. Suppose, for example, a dog poisoned by lead compounds to be subjected to the treatment by iodide of potassium, and the resulting double iodide passing by the liver and the gall-bladder to be just poured into the intestine, in order to be returned from thence into the system,—we may conceive at once the utility of purgatives, and of the sulphate of magnesia, especially, associated with the iodide of potassium. I should not have put forth this view, if facts had not warranted me in believing that in certain cases the phenomena may take place in the order above stated, and if it were not of advantage to practical medicine to clear up this question.

§ 1. *Principle of the treatment by iodide of potassium. To render soluble any metallic compounds which have become fixed in the living body, and to facilitate their elimination by uniting them with a substance most readily cast out of the system.*

Such is the principle of the treatment by iodide of potassium of the disorders due to mercurial and saturnine poisoning. In a chemical point of view, it differs essentially from any plan of treatment hitherto proposed. . . . . It has been generally supposed that mercury and lead are present in the body in these cases in the form of soluble salts (a supposition which, for lead especially, is very doubtful); and many remedies founded on chemical views had for their object to form insoluble compounds with the poisonous substance, such compounds being thought to have no action on the living economy. I shall, however, prove, farther on, that the sulphate of lead, for example, is so far from being without action on the living body, that its presence inevitably causes death within a limited period—a fact which leads to a distinction between slow and rapid poisoning.

According to the old chemical view, therefore, the object was to render insoluble a poison which was supposed to be previously soluble. The object of the treatment by iodide of potassium, on the contrary, is to render soluble metallic compounds that have become fixed in the body, to the end that, being again in a state to be taken up by the blood, they may be cast out of the system.

§ 2. *Harmlessness of iodide of potassium in the dose of from* 30 *to* 92 *grains (two to six grammes*) a day for an adult.*

I took in the course of two months about 2315 grains (150 grammes)

* One French gramme is equal to 15.438 English grains. In converting the gramme into grains, in this and other instances, I have ventured to drop the decimal figures occurring in the English scale, any quantity less than a grain, where such considerable weights are dealt with, being plainly immaterial as affecting the dose.—TRANSLATOR.

of iodide of potassium. I began with 30 grains (two grammes) a day, and went on increasing the dose, with an occasional interruption of a day or two, up to 92 grains (six grammes), or rather more than a drachm and a half a day.*

This treatment caused neither pain nor disorder of any kind. I had coryza for the first few days; some pimples on the skin; a peculiar taste in the mouth, without metallic after-taste: my appetite increased.

§ 3. *The kidneys are the principal outlet of the iodide of potassium. It is even with extreme difficulty that this salt can be made to pass through the bowels into the stools.*

The presence of iodine was very readily detected in the expectoration, in the perspiration, in the saliva, and in the tears. The mucus of the nose contained it, but in very small quantity. My surprise was great when, on examining the fæcal matter of two patients who were taking iodide of potassium in the dose of 77 grains (five grammes) a day, I could not detect the slightest trace of iodine. Their urine contained it in large quantity.

I macerated two pounds of fæcal matter in water; the water was examined by the ordinary tests—starch, an acid, and chlorine,—but without result. As the proper reaction might possibly be hindered by organic matters, I calcined two pounds of fæcal matter procured from another patient, and rendered alkaline by the addition of potash, but could discover no trace of iodine.

I examined my own fæcal matter four days in succession, while I was taking 92 grains (six grammes) of iodide of potassium daily; operating on about 3087 grains (200 grammes) of moist fæces, which were carefully calcined after having been made alkaline by potash. In the first trial I found no trace of iodine; in the second and third slight traces were discovered; in the fourth, again, there were none.

I wished to ascertain next in what manner the iodide would be eliminated when associated with an active purgative. After using an enema, I took 926 grains (60 grammes) of sulphate of soda, and 61 grains (four grammes) of iodide of potassium. The saline residue of three copious stools thus produced contained but a very small quantity of iodine, whereas the urine was much loaded with it. A fourth stool, passed twelve hours afterwards, contained not a single trace of iodine. The principal outlet of the iodide of potassium is, therefore, the kidney.

* This will probably seem to the British practitioner a very large dose. I have often seen troublesome disorders occasioned by a much smaller quantity. I believe, however, that the dose mentioned by the author may be safely given in most cases, provided two precautions are observed. One is, to take the remedy fasting; the other, to take it largely diluted. It is difficult to say which of the two precepts is the more important. A painter is now under my care who has taken two drachms a day of the iodide for the last month, with little other sensible effect beyond increase of appetite and a progressive gain of flesh and strength. On the whole, there can be no doubt that the doses generally given in this country are too small to admit of the full virtues of the drug being brought into play in every case. For this reason, I believe that these virtues are not even yet estimated as highly as they deserve.

In the treatment of rupia and of syphilitic ulcer of the throat, of phagedenic character, I have often seen this strikingly exemplified. One or two cases in particular, of rapidly destructive ulceration of the throat, occur to me, in which, under the employment of the iodide in the dose of fifteen grains a day, the ulceration continued to make frightful havoc, but began to *heal rapidly* when the dose was raised, first to half a drachm, and then to two scruples, and then to a drachm in the 24 hours.—TRANSLATOR.

§ 4. *Every mercurial compound which can possibly occur in the living economy, even metallic mercury itself, is soluble in iodide of potassium; the presence of the organic substances of the body does not hinder these reactions.*

It is easy to understand the motives which led me to propose iodide of potassium for the treatment of the disorders produced by lead and mercurial poisoning, especially when these disorders occur in a chronic form. Let us first consider the case of mercury.

It is well known that persons who have undergone mercurial treatment have observed, even after an interval of many years, that gold placed in contact with their perspiration has become coated with mercury, especially when excessive perspirations have been caused by the use of the vapour bath. If this fact be true, it proves that the system may absorb and retain mercury for a long time under forms which I will not try to define, but which very probably result from the insoluble compounds which the salts of mercury form, either with the organic or inorganic materials of the body, or with both conjoined. Perhaps the mercury may even occur in the metallic state, as some have admitted; at any rate, it is present in the body in such form as to be retained there. The principal combinations which might thus occur may be reduced to the following:—

1. Combinations of corrosive sublimate, whether in its simple state, or as modified by the animal substances of the economy—namely,
   a. With albumen.
   b. Albumen, and the materials of the brain.
   c. Gelatine.
   d. The nitrogenous extractive matters of the blood, of muscle, of the urine, &c.
   e. Albumen, fibrin, muscular fibre, gelatine, whether in the natural state or modified by digestion.
   f. Matters of the bile.
2. Mercurial soaps.
3. Phosphates of mercury.
4. Mercury in the metallic state. (?)

All these compounds are soluble in alkaline or neutral pure iodide of potassium dissolved in one of the liquids of the body. I have made experiments with each of the compounds here enumerated, and have always succeeded in dissolving them under whatever circumstances. If the iodide of potassium be associated with a dilute acid which has no energetic action either on the solution of the salt or on the principles which occur in the body, the solution of the fixed mercurial compounds is still effected perfectly. In obtaining this last result I operated with lactic acid. After fixing corrosive sublimate on nervous filaments, muscular fibre, or on tendons, it is only necessary to wash them for a short time in a solution of iodide of potassium, whether acid, alkaline, or neutral, in order to remove entirely the mercurial salt. It is especially with a solution of albumen and sublimate that these properties admit of being perfectly demonstrated; indeed, the experiment has been for many years a class-experiment in M. Dumas' course at the School of Medicine. It is only

necessary to pour a solution of iodide of potassium on the precipitate formed by albumen and sublimate, in order to see the liquid become instantly limpid. The iodide of mercury possesses a property which I must not omit to point out. This compound, as is well known, is soluble in caustic potash. Now, although caustic potash is not found in the living body, the alkalinity of the greater number of the fluids which are found there is worthy of being borne in mind, and acquires a certain degree of interest when viewed in relation to the following experiment. Mercury coarsely divided was placed under a layer of water holding in solution caustic potash and iodide of potassium. After some weeks' contact a considerable quantity of mercury was found dissolved in the liquid.

It is well known that metallic mercury in contact with alkaline chlorides in solution, itself passes in part to the state of chloride. It remained for me to prove that this reaction might occur in perfectly neutral or even in alkaline fluids. It was necessary in this experiment to guard against the intervention of the carbonic acid of the air, or of the acids sometimes diffused in the air of chemical laboratories. It appeared to me quite necessary to make this experiment, especially as it was possible that mercury might exist in the body in the metallic state, and yet act as a poison; in which case, the experiment I have just cited would still permit the hope that the poison might become dissolved by the action of the iodide of potassium, rendered alkaline by the fluids of the living body.

When metallic mercury is shaken in a solution of iodide of potassium, whether neutral or slightly acidulated with hydrochloric acid, the solution soon acquires an alkaline reaction—an incontestable proof that the oxygen of the air has laid hold of the potassium of the iodide, which, in its turn, has yielded its iodine to the mercury, the iodide of mercury thus formed having entered into combination with the iodide of potassium remaining in excess. It suffices to shake vigorously a neutral or slightly acidulated solution of iodide of potassium with an excess of mercury, for the space of a minute, in order to see the reaction occur. This might be easily done as a class-experiment, to show the tendency of the alkaline haloid salts to form double salts with the corresponding haloid salts of the metals properly so called. An analogous phenomenon may be observed, although less readily, with common salt, and, indeed, simple potash excites the oxidation of mercury, and dissolves small quantities of the oxide. When these experiments are made in closed vessels, it is easy to demonstrate the disappearance of oxygen, by analyzing the air which has been operated on.

§ 5. *Rapidity with which the iodide of potassium traverses the system.*

If these properties of the iodide of potassium seem remarkable when standing alone, they acquire a new interest when they are confronted with the phenomena which this salt offers to us in its passage through the body. In fact, if the alkaline iodides have a very great tendency to unite with the metallic iodides, whether associated or not with organic matters, the former possess the property of passing off in the urine with extreme rapidity, and, when they are pure, the body rids itself of them in a very short time. Is it not probable that the elimination of the alkaline iodide is accompanied by the elimination of the double or triple compounds which it is so easy to produce in the laboratory?

I attach some importance to the demonstration of the rapidity with which the iodide of potassium passes from the stomach into the urine, and of the very short time which is required in order that the system may be entirely rid of it. The following experiments prove it:

A person, after emptying his bladder, took 77 grains (five grammes) of iodide of potassium. A few minutes afterwards iodine was detected in the urine. This experiment has been repeated, and iodine always detected in the urine passed on the first occasion of a call to make water.

The following experiment shows the rapidity with which the economy gets rid of the greater part of the iodide taken in. I took 679 grains (44 grammes) of iodide of potassium in eight days, 77 grains (five grammes) daily in four days, and 92 grains (six grammes) in other four days. After having ceased to take the salt, I tested my urine, every time I voided it, with starch, an acid, and chlorine, and on the second day after that on which I ceased to take the iodide, I could no longer detect its presence, although the most minute portion of iodide added to the urine was made manifest at once.

Are the double iodides eliminated as easily, and in the same manner? There is every reason to believe so. However that may be, I have thought this experiment worthy of attention.

§ 6. *It is not possible to give directly the same accumulated proof of the solution of lead, as of that of mercurial compounds.*

All that has been said of the solubility of mercurial compounds in iodide of potassium becomes less clear and more difficult to prove when we have to deal with the compounds of lead. I shall content myself with observing, therefore, that the iodide of lead is soluble in alkaline liquids, and that it has a marked tendency to combine with alkaline iodides. These facts have appeared to me to constitute adequate motives to induce medical men to employ the treatment by iodide of potassium for complaints which, however much they may be relieved, are, according to our best physicians, rarely, if ever, radically cured.

I have proved that metallic lead becomes dissolved in a solution of iodide of potassium, rendered alkaline, by potash. It is well known how rapidly certain metals become oxidized when exposed to moist air, in the presence of even feeble acids, like the carbonic acid of the atmosphere. This is the case with iron, and indeed with lead. The intervention of an alkali sometimes suffices to prevent oxidation. Such is the case with iron. Lead, copper, and zinc become oxidized, on the contrary, more rapidly in contact with an alkaline liquor. I have proved that granules of perfectly metallic lead, bathed by a solution of iodide of potassium, rendered alkaline by potash, become, after a time, partly dissolved in this mixture.

§ 7. *Lead colic comes on more especially after the process of washing in the "second water," ("lavage à l'eau seconde.")*

The property which metallic lead possesses of being easily attacked by alkaline liquids exposed to the air has appeared to me the more worthy of remark, because it seems still to be generally admitted that lead occurs in

the form of a salt in the living body, whereas there is every reason to believe that it exists there in a masked condition (état dissimulé), or rather, in the form of plumbate of soda. I have several times been struck with a fact which, to a certain degree, renders this last assertion probable. Painters, on being carefully questioned, with the view of fixing accurately the date of their attacks, and the circumstances under which they occurred, very often answer, that they were seized with their pains after washing old paint. Now it is well known that this process is always done with alkaline solutions.

§ 8. *Succedanea of the iodide of potassium for diseases caused by metallic poisons.*

I do not think it necessary to refer to other alkaline iodides, earthy or metallic, as substitutes for iodide of potassium. The action of the other iodides, although capable, possibly, of varying the energy of the treatment, must be at bottom the same.

The property which many salts of an alkaline base possess of combining with the corresponding salts of a metallic base, warrants the supposition that the chlorides, bromides, &c., might be employed with effect in the treatment of these terrible maladies. An excess of common salt in the ordinary food seems already to be useful as a prophylactic. Thus, in seeking information as to the habits of life of those workmen who have escaped the scourge of these disorders, or on whom it has fallen lightly, it has often occurred to me to ask if they were fond of salted food. Those among them who told me they were fond of salt, had either suffered less, or not at all. I had occasion to make this observation in the case of ten workers in mercury. It is one which well deserves to be tested by the medical practitioner. In this case, does salt act as an excitant, or does it act, as one might suppose, in rendering the mercurial compounds at once more stable and more soluble, and thus favouring their expulsion?

The double cyanide of potassium and iron, which may be taken with impunity in a large dose, at least, during several days (as I have again shown in the case of dogs), might also be useful, especially in the disorders due to mercury.

§ 9. *Cases of lead poisoning.*

Let us examine now the facts on the strength of which I would induce medical men to employ iodide of potassium, in the chronic disorders produced by poisonous metallic compounds.

1. M. Boucher, house-painter. Saturnine pains in the spine. (Rachialgie). Incomplete paralysis of the arms. Lead colic. Treated without success at the Charité, under two different physicians; also, at the Hôpital Cochin.

From the 10th December to the 13th March, 1844, he took 3087 grains (200 grammes) of iodide of potassium. At the last-named date he was perfectly cured.

This patient was treated at his own house. As soon as he could take nourishment he was allowed to follow his usual mode of living. He had at his command a graduated solution of iodide of potassium. He began with a small dose; he stopped now and then for some days, increasing or

diminishing his dose according to his pains and sensations. On the 27th of May he had already gone to work, and continued well in spite of the weakness of his forearms. Frictions, with an ointment containing iodide of potassium and a little carbonate of soda, were now ordered. He derived benefit from them, and if the paralysis did not entirely disappear, he at least recovered strength in his hands.

At the moment when the metallic compounds fixed in the body become dissolved or transformed, phenomena of acute poisoning may occur, caused by their liberation. These phenomena were, at the outset, so intense in the case of Boucher, that the treatment might have been supposed to be hurtful rather than beneficial. But the occurrence of these phenomena affords incontestable proof of the energy of the treatment: the dose of iodide which excited them was only 46 grains (three grammes) a day.

At the commencement of his treatment the patient's urine was of a deep brown, and had been so for some time, but after the first few days this colour disappeared, and the urine acquired the tint of common urine. I have made the same observation in other cases.

2. A typefounder complained of *tormina*, and of weakness of the legs. 2778 grains of iodide of potassium entirely restored him.

3. A workman, of forty or fifty years of age, who had worked in a white lead manufactory, and also in an establishment for making visiting cards, had been in hospital more than six weeks. When first subjected to the iodide of potassium treatment he was weak and quite broken up.

It is almost impossible to form an idea of the rapidity of the amendment which ensued in this man. He grew better, so to speak, as you stood by. At the end of three weeks he left the hospital completely cured. The dose of the iodide had been raised pretty rapidly: when he left the hospital he was taking 77 grains (5 grammes) a day.

4. A man about fifty years old had been subjected to different modes of treatment, without benefit. When he began his treatment he could scarcely hold himself up; all his limbs were more or less palsied; he was pale and emaciated. After five or six weeks' treatment he was perfectly well, and left the hospital at his own request. When his treatment began his urine was of a deep brown; a fortnight afterwards it had the colour of common urine.

5. Ordinarily, dogs and cats die within a very short time in the greater number of establishments where lead and its compounds are worked: rats and mice do not harbour there. All animals exhibit symptoms analogous to those which are observed in man. A bitch of middle size kept guard on a terrace covered with lead. In the month of September, 1840, she poisoned herself by eating a bladder, in which painters' colours had been kept. She was seized with colic, vomiting, &c. She was cured, but remained thin. The year following, at about the same time, she again had colic, vomiting, &c.; and although she recovered she became from that time subject to similar seizures. These phenomena recurred four or five times a year, always with the same symptoms, which varied only in severity. On the 25th of October, 1843, while in one of these attacks, she was subjected to treatment by iodide of potassium, which was mixed with her food. In forty-nine days she took 3087 grains (200 grammes), distributed in the manner following:

24 days,	31 grains	(2 grammes)	per day.
10 „	61 „	(4 „)	„
7 „	92 „	(6 „)	„
8 „	123 „	(8 „)	„
49			

The administration of iodide of potassium may, then, be carried to a high dose, for the weight of the bitch was probably only about 20 lbs. (10 kilog.) While the animal was taking the highest dose her sight was enfeebled, but it had completely recovered a few days subsequently. After having undergone this treatment, the bitch had not only entirely recovered, but was better than she had ever been. During the year which preceded her treatment she was very thin: immediately after her last attack her emaciation was extreme. After having taken 3087 grains (200 grammes) of iodide, she had become positively fat; pending the treatment her appetite was voracious. The 30th March, 1844, she was seized with a fresh attack, but much less severe than the former ones. She howled less, and did not get so thin. The 3rd of April she again began the treatment by iodide of potassium, in the following manner:

10 days,	31 grains	(2 grammes a day,	20 grammes,)	310 grains.
10 „	61 „	(4 „	40 „)	610 „
10 „	92 „	(6 „	60 „)	920 „
30			120	1840 grains.

She was again cured.

There was every reason to believe that this malady, which recurred periodically, was due to contact with the lead of the terrace. I have not been able, through circumstances, to put this conjecture to the test; nevertheless, on removing her from the influence of the lead, she lived six months without exhibiting any symptom of the disorder.

§ 10. *Neither sulphuric acid, nor sulphates, can serve as antidotes to slow poisoning by the salts or compounds of lead, the sulphate of lead being itself a slow but sure poison capable of killing vigorous dogs in 20 or 30 days.*

[In support of this assertion, M. Melsens remarks that the evidence commonly adduced is not sufficient to prove sulphuric acid to be an antidote to slow lead poisoning, although sulphate of magnesia may be properly given in cases of poisoning by a soluble salt of lead to act on the portion yet unabsorbed. Sulphate of lead, however, may be, and probably is, less readily absorbed than other compounds; but that sulphuric acid will reach lead already deposited in the system is quite unproved. Moreover, sulphate of lead is itself a poison, as can be shown by experiments on dogs, some of which are detailed. One dog was paralyzed on the 11th day after 108 grains of the pure sulphate; he then refused food, and died epileptiform, emaciated, and in a state approaching scurvy on the 22nd. Another dog took 293 grains, and died on the 28th day, with similar symptoms.—*Translator.*]

§ 11. *When a great excess of sulphate of lead is administered, the phenomena of lead poisoning are not in ratio to the quantity of poison administered.*

[Experiments on dogs are cited to prove this. It appears that only a certain portion can be absorbed—the rest passes with the excrement.—*Trans.*]

§ 12. *The sulphate of lead obtained by double decomposition washed and ground, is as poisonous as that obtained by precipitating from a soluble salt of lead with sulphuric acid.*

[The only difference observed was, that the quasi-scorbutic symptoms appeared more slowly, and ulceration of the cornea occurred.—*Trans.*]

§ 13. *If to a dog that has been for some time under the poisonous influence of sulphate of lead, iodide of potassium be administered suddenly in pretty large doses, death will ensue. If, on the contrary, the two drugs be given concurrently, the dog will suffer no harm. Iodide of potassium may be employed therefore as a prophylactic.*

[Experiments on dogs very carefully made prove this assertion, and show "that the minute quantity of lead which traverses the body disappears without causing much harm if it meets with a sufficient quantity of iodide of potassium to favour its expulsion. When, on the contrary, lead compounds accumulate in the body of the animal, death is inevitable."—*Trans.*]

§ 14. *Conditions required for the cure of a saturnine affection by the iodide of potassium.*

A little bitch, about a year old, was subjected to poisoning by the sulphate of lead in the dose of 15 grains (one gramme) a day. At the end of eight days, she had lost much flesh; the scorbutic affection was strongly marked; the muzzle stank; there was distinct paralysis of the limbs. She only weighed 3 kil. 915. The sulphate was continued a few days longer, so as to aggravate the disorder without killing the animal. After eight or ten days the administration of the poison was interrupted, and the bitch was let alone, but she still continued to waste for a fortnight longer, in spite of every care, although her appetite was tempted with milk and flesh. She would, however, scarcely take any food.

On the 9th of October death seemed impending. The emaciation was extreme—the muzzle fœtid to the last degree—the paralysis almost entirely prevented walking—the spinal column was curved so as to represent about the third of the arc of a circle: the animal was cold. In this state she weighed 3 kil. 025: so that, without counting the first loss, of which no record was kept, the animal had lost in a month about one-fourth of her weight.

While in this state she was put under treatment by iodide of potassium, commencing with rather more than 7 grains (half a gramme) a day. The dose was gradually increased as the animal seemed able to bear it; until at the end of 27 days, from 61 to 77 grains (four or five grammes) of iodide were taken daily. During the whole treatment 1Q03

grains (65 grammes) of iodide of potassium were given. From the very first day or two there was a change in the disorder. The appetite returned, the emaciation and paralysis disappeared, and on the 27th day the animal might be regarded as perfectly cured. A few days more, and there remained no trace of her poisoning. Nothing could be more remarkable than the alternate loss and recovery of weight, succeeding one another in so short a time, and indicating with so much fidelity the progress of the poisoning and the salutary effect of the antidote.

The following are the weights of the bitch pending her cure, taken in the mornings when she was fasting:

		Kilog.
9th of November	. . . .	3·025
14th „	. . . .	3·240
19th „	. . . .	3·404
22nd „	. . . .	3·850
25th „	. . . .	4·168
1st of December	. . . .	4·682
6th „	. . . .	5·188

From the 6th to the 13th of December she remained of the weight last quoted. Here, then, is an instance of an animal becoming much emaciated in ten days, under the influence of a salt of lead: losing, again, one-fourth of its weight during the following month, under the same influence; but, on the contrary, doubling its weight, or nearly so, after 27 days' treatment by iodide of potassium.

In medical practice physicians have to treat painters and other workmen, who generally have been a long time under the influence of the poison. They will not always see the complaints, and the paralysis especially, disappear so easily, rapidly, and completely, as in the case I have just reported.

M. Magendie has also proved that fibrin of new formation is more easily attacked by re-agents than fibrin of old formation. We need not be astonished, therefore, if in workmen who have been a long while affected, the compounds of lead, fixed in the system from a remote period, should resist the action of solvents more strongly than combinations of recent formation.

§ 15. *Aggravation of the morbid phenomena after the administration of iodide of potassium in dogs that are under the influence of lead compounds:—harmlessness of the same doses of iodide of potassium in healthy dogs.*

If compounds of lead are administered to animals, and, after the resulting disorder has reached a sufficiently advanced stage, the mischief becomes aggravated by the employment of iodide of potassium, *this aggravation is to my mind a sure token of cure, for it proves that the remedy is acting.*

It was necessary to show that the same thing occurred with other salts of lead than the sulphate. In order to prove this I had recourse to white lead, the substance which gives the disease to painters, although this same aggravation of the disease had been perfectly observed in the first patient mentioned in this memoir. (Case of Boucher.)

A dog of middle height took 123 grains (eight grammes) of the impure white lead of commerce in eleven days. He exhibited much the same

morbid phenomena, but occurring here rather more rapidly than those produced by the sulphate of lead. On the fifth day the appetite was gone, and there was vomiting—a symptom which had not been observed in dogs treated by the sulphate. On the eleventh day he was powerfully affected, and would take absolutely nothing until the fifteenth day, when he again swallowed rather more than 7 grains (0·50 grammes) of the poison. On that day 46 grains (three grammes) of iodide of potassium were given to him. A short time afterwards the morbid phenomena were aggravated in an extraordinary way.

Some hours afterwards the dog was sad, his eyes were half closed; there was extreme prostration and trembling of the whole muscular system. He seemed to be in a state of drunkenness; the sight was dim; the head was carried on one side as from vertigo: there were epileptiform convulsions—he staggered and fell, to rise again with a more tranquil look; but the convulsions soon after returned, to disappear again.

On the morrow, although the phenomena were much the same as the evening before, 92 grains (six grammes) of iodide were again given. The dog's appetite seemed excited. he seized and ate the food offered to him. On the day after the morrow, after the administration of 123 grains (eight grammes) of iodide, the hope of recovery seemed so small that on the fourth day the dose was only carried to 61 grains (four grammes). The dog, nevertheless, died on the next day, at five in the morning.

The dangerous phenomena which supervene on the administration of iodide of potassium, in cases of lead poisoning, cannot be too strongly insisted on, as showing the necessity of great caution in the employment of this remedy in man, for the first few days.

I took a dog of about the same stature and strength, as the preceding one, and I gave him the same quantity of the same iodide of potassium, administered at the same hours. These 324 grains (21 grammes) of iodide of potassium administered in four days to the second dog, which weighed 5 kil. 500, produced no injurious effect; there was only a slight vomiting on the second day.

Another dog of the same size took in twenty hours 77 grains (five grammes) of iodide of potassium, and appeared to be in no way inconvenienced by it.

§ 16. *Action of iodide of lead on the living economy.*

In what way does the iodide of lead, itself, act on the living economy? 115 grains (7·50 grammes) of this salt, administered in the dose of one gramme at a time in the course of seventeen days, caused the death of a dog. Most of the resulting morbid phenomena resembled those produced by the sulphate and carbonate of lead, except in the rapidity of their appearance. This animal had convulsions and fits from the sixth day, having then taken about 77 grains (five grammes) of the salt. Iodide of lead is, therefore, a more active poison than the sulphate, being nearly on a par in this respect with the carbonate. But this is not all we would desire to know.

Is this poison, administered as in the preceding case, less active when associated with the iodide of potassium?

Is it possible, when the poison and antidote are already administered in

poisonous doses, to aggravate the phenomena of poisoning by pouring an excess of iodide of potassium into the system?

The following is an experiment which answers this question. To be conclusive, however, it should be varied and repeated with the drugs in other proportions.

A bitch, of the same size and strength as the preceding, took in thirty-three days, and generally in doses of 15 grains (one gramme) at a time, 216 grains (14 grammes) of iodide of lead. With every dose of iodide of lead there were given 30 grains (two grammes) of iodide of potassium. In thirty-three days she had taken 370 grains (24 grammes) of this latter salt. This animal wasted much less than the former one: the paralysis, although easily recognised, was much less marked; to judge from her melancholy appearance she was, however, very seriously affected. She died in a few minutes, after having taken 92 grains (six grammes) of iodide of potassium, which had been rendered slightly acid.

§ 17. *Cure of gilders, and of workers in quicksilver.*

The first patient subjected to the treatment was an old man at Bicêtre, whose case has been recorded with full details by M. Guillot. He had been a gilder, and had lost the use of his limbs by the exercise of his calling. His sight was gone, and he was, moreover, tormented by violent and constant pains. Iodide of potassium was administered every day, in the dose of 61 grains (four grammes). At the end of a fortnight's treatment he left the infirmary alone and without help, whereas on his admission he was obliged to be brought in a litter.

2. I saw a gilder in mercury, about twenty-five years old, treated by the iodide of potassium during three weeks. At the end of that time he could write with ease, whereas at the beginning it was perfectly impossible for him to write at all.

3. The only person on whom the iodide of potassium did not act with energy was an old man of seventy, a working gilder. He was only treated during a few days for a strong salivation; the treatment was given up.

4. Barthelemy Pichon was a working gilder. His memory was very feeble; his speech much embarrassed; and the tremors were so violent, that he could only be employed in the coarser operations of water or galvanic gilding. He belonged to the factory of M. Christofle.

In the beginning of 1844 he was six weeks at the Hôtel Dieu, but with small result. The 1st of June, 1844, I let him have 3087 grains (200 grammes) of iodide of potassium in solution, recommending him to begin by taking a portion corresponding to 15 grains (one gramme) of the salt daily, increasing the quantity as he liked; guiding himself by the severity of his pains, and diminishing the dose when these pains should become too severe. After a few days' treatment he was seized with pains in the head and stomach, and had pimples over the body,—symptoms which did not, however, prevent him from working constantly during the seventy days that his treatment lasted, and that were taken up in consuming the 3087 grains (200 grammes) of iodide of potassium. He followed his ordinary regimen, and changed his habits in nothing.

After the treatment, Pichon was completely cured. He wrote without

shaking; he spoke glibly. According to the report of M. l'Eté, this workman, who before his treatment was unable to execute any but the coarsest works belonging to gilding on metal, after seventy days' treatment executed the most delicate processes. There was intense headache on one occasion during his treatment, but this was in sequel to a debauch, after which he had taken no medicine for three or four days. In order to recover the lost time, he took at one draught a dose which should have been distributed over several days. Immediately after this dose the pain of the head was so agonizing, that he thought he should have gone mad.

5. A looking-glass maker, employed in one of the Brussels manufactories, put himself under treatment by iodide of potassium. Before he became a gilder he had been a typographer. He trembled to such a degree as often to drop everything he held in his hand; his speech was slightly embarrassed; his legs supported him ill; he often made false steps; his strength failed him almost entirely. Three hundred and eight grains (20 grammes) of iodide of potassium, taken in ten days, scarcely affected him. As he belonged to a provident society of operative typographers, the medical attendant of the society desired, before continuing the iodide of potassium, to subject him to other treatment. He was of opinion that trembling of another character might complicate the mercurial trembling. A month afterwards he sent the patient back to me in exactly the same state as before. He was again placed under treatment by the iodide. Although this artizan had left his trade, the amendment following the treatment was very slow. He told me, nevertheless, that he felt his strength return daily: the tremors gradually disappeared. It was not, however, until he had taken 3087 grains (200 grammes) of iodide that the amendment was very marked. He scarcely trembled at all, his step was firm, his handwriting good; but his strength had not entirely returned. Observing that the cure in this patient was progressive, but slow, in spite of his withdrawal from his workshop, I ordered iodide of potassium to be used in frictions, under the form of iodine ointment. It was especially from this time that the recovery of strength became perceptible. It was not until towards the end of August, after three months' treatment, that he had entirely recovered.

I saw this workman again in the beginning of the month of November. He told me that his health and strength were much as they had been before he began the business of a looking-glass maker. In the month of April, 1849, he was perfectly well, and nothing remained of his old complaint.

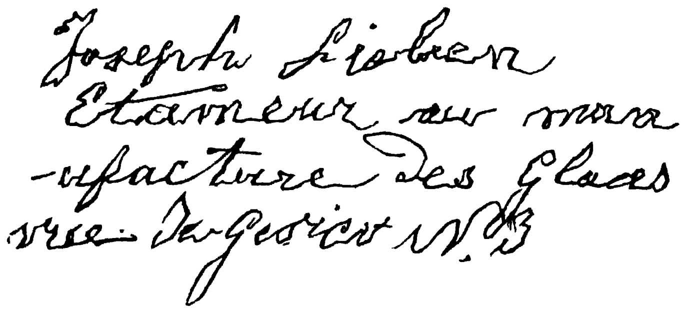
This man only remained proof against the pernicious influence of the trade twenty-one months. Having entered the manufactory in March, 1846 the trembling came on in January, 1848. In questioning him in the presence of many of his comrades I made this observation, which appears to me important:—Those workmen who are fond of salt resist the contamination longer than those who are not.

6. An operative in the same factory was affected with a trembling, which embarrassed him much in his work. His limbs were exceedingly weak; the slightest effort fatigued him. Sometimes objects dropped from his hands: his gait was insecure. His tremors were already of old standing: a year before, he was ill enough to keep his bed, and although relieved then, he remained weak after the treatment which was adopted.

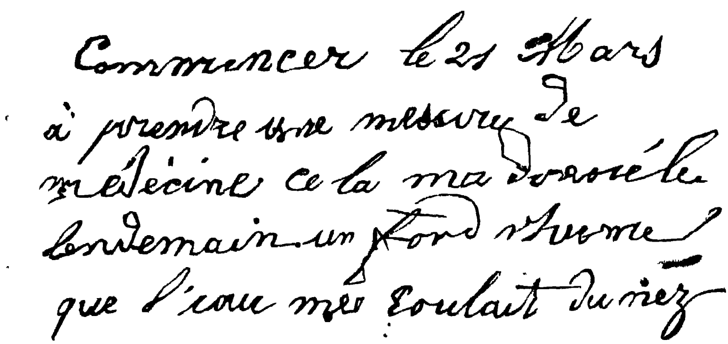
From the 21st of March, 1848, to the 7th of May, he took 771 grains (50 grammes) of iodide of potassium. During the whole of this time he continued to work at silvering, as before, and, what is more important, his work in contact with mercury had almost doubled in the number of hours. Nevertheless, on the 7th of May the tremors were almost reduced to nothing. He took afterwards, up to the 22nd of June, about 1543 grains (100 grammes) of iodide of potassium. He often interrupted the use of the salt for a few days, taking it according to his own judgment, the severity of his pains being his guide. The 4th of September he was perfectly well; trembled little or not at all, and continued his calling without any recourse to medicine. During the whole of this time he never ceased to work, and was always in contact with the poison. His cure was accomplished without preventing him from gaining his livelihood and maintaining his family, and without any change in his regimen or habits.

Nothing is more curious than to examine a portfolio in which the patient wrote to me every day a few words. A fac-simile of some lines taken at hazard will give an idea of this:

1. Writing of the 1st of March previous to the treatment.


Joseph Lisben
Etameur au manu-
-ufacture des Glaces
rue [illegible] N° 3

2. After three or four days' treatment.


Commencer le 21 Mars
à prendre une mesure de
médecine cela ma donné le
lendemain un fond rhume
que l'eau me coulait du nez

3. Writing of the 5th of April.

J'ai eu un gout dans ma
bouche tous long du jour
Comme ci je manger de
Cuivre.

4. Writing of the 7th of April, after having taken 262 grains (17 grammes) of iodide of potassium in seventeen days.

Apres avoir pris 34 mésures
De la médecine sur 17 jours
et Souffert un peu Des Douleurs
Dans mon corps, Tous mon
tremblement étois passé je
me trouvé beaucoup mieux
la mine changer et la force
plus fort Dans les Membres

Joseph Liebers

5. Writing of the 5th of June, after having taken 1543 grains (100 grammes) of iodide of potassium.

Pendant mon interruption je m'ai toujour bien porter. mais les dernier jour ~~j'ai~~ j'e m'ai sentï trembler un peu plus fort que l'habitude -

6. Writing of the 12th of April, 1849. He has continued to work at silvering since the month of March last.*

Bruxelles les 12 Avril 1849

Depuis le mois juillet 1848 j'ai été guéri et je n'ai plus rien aperçu du tremblement jusqu'à ces jours.

J Lieben

At the beginning of the treatment, the urine of this patient exhibited twice a somewhat uncommon phenomenon. It was limpid, of a bright yellow, but alkaline to such a degree as to effervesce with acids. After a fortnight's treatment, this phenomenon did not recur.

I examined, on eight occasions, four pints of the urine of this patient, in order to see if, under the influence of the treatment by iodide of potassium, the mercury would be found again in his urine. The first

* In the original, two other specimens are inserted of the writing, the 5th of May and 7th of August, but the steps of improvement are sufficiently shown by the above.

specimen contained mercury in perfectly recognisable quantity; the second also contained mercury, but sensibly less.

In following the process employed for the first two specimens, I found no mercury in the third. The fourth specimen, examined by another process, gave scarcely an appreciable trace; but on distilling, in a tube drawn out into a point, one of the slips of gold which terminated the pile I had used, I obtained a small yellow sublimate, which on the morrow had become red. In dispersing this by the flame of a spirit lamp, it became again sublimed with a yellow colour, passing afterwards into red. This is one of the characteristic properties of iodide of mercury.

Is it not remarkable to see a man under the influence of slow poisoning by metallic mercury, void, under the influence of iodide of potassium, iodide of mercury in his urine?

The patient now and then interrupted his treatment. I examined the urine a fifth time during one of these interruptions. I failed to discover mercury in it, but at this time the patient might be considered as cured.

I examined three other specimens of urine, collected when he was taking the strongest doses of iodide of potassium; but whether it was that the urine contained none, or contained it in quantity too small to be detected by the means employed, I could no longer detect any mercury. However that may be, this operative, who continued to work in quicksilver, and who, since the end of June, had taken no more medicine, continued perfectly well. At the beginning of the month of November, all his functions were performed well; his strength and vigour had returned. The organs of generation, previously for a long time passive, had recovered their former energy after some weeks' treatment, and had maintained it since.

This cure proves in the clearest manner, that when mercury is only absorbed in small daily portions, as is the case with those who work at the cold silvering process, iodide of potassium not only possesses the property of curing the patient, but acts also as a powerful prophylactic.

§ 18. *Experiments tending to prove that the iodide of potassium protects against or retards the phenomena of poisoning when the system is subjected to the action of metallic mercury.*

[Experiments are cited to show that, given with mercurial ointment, an excess of iodide of potassium retards, if it does not prevent, fatal results. M. Melsens remarks, however, that although iodide of potassium may act as a prophylactic to metallic mercury, in other cases the iodide of potassium may, instead of relieving, be a dangerous remedy.—*Trans.*]

§ 19. *Experiments showing that the iodide of potassium renders medical treatment or poisoning by certain salts of mercury more active, and may occasion serious accidents.*

For a long time past (from the year 1838-39 at least) M. Dumas has enjoined, in his lectures, the importance of abstaining from common salt when calomel is given, and is desired to remain in the state of calomel in the digestive tube. He has recommended, on the contrary, the addition of sal-ammoniac to corrosive sublimate, in order to render it more stable and less easy to be decomposed when in contact with the fluids

of the system. The work of M. Mialhe is sufficiently well-known for me to content myself with a simple reference to it. If the same doses of calomel are given to two dogs, and to one iodide of potassium is given at the same time, the dog which takes the iodide will be destroyed first if the doses of the two salts are at all large. In a comparative experiment made on two dogs, chosen of the same age and strength, as near as may be, I obtained the following results. The first took seven grains (0·500 gramme) of calomel in all, and about one drachm (four grammes) of iodide of potassium administered in the following manner:

First Day.—Rather more than three grains (0.200 gramme) of calomel and 30 grains (two grammes) of iodide of potassium.

Second Day.—Nothing.

Third Day.—Rather more than four grains of calomel (0·300 gramme) and 30 grains (two grammes) of iodide of potassium.

He died in the night of the fifth day. The same doses of calomel without the iodide scarcely affected the second dog at all. On the day of the death of the former he still took about one and a half grain (0·100 gramme) of calomel. He was then let alone for seven days. Nearly two and a half grains (0·150 gramme) of calomel did not prevent him from living five days longer. He had taken, therefore, twice as much calomel as the first dog, and had lived twelve days longer. He suffered, however, a good deal. I gave him at one dose as much calomel as was taken by the first in three days—namely, nearly eight grains (0·500 gramme), and on the morrow 30 grains (two grammes) of iodide of potassium. The symptoms grew worse: he died forty-eight hours afterwards, without any fresh administration of either calomel or iodide.

§ 20. *The action of iodide of potassium on a dog treated by corrosive sublimate may be so energetic, that even eight days after he has taken the sublimate, a pretty large dose of iodide of potassium will prove fatal to him.*

I desired to ascertain whether, in subjecting a dog to treatment by the sublimate, and then interrupting the treatment during some days, it would be still possible to recognise the energetic action of the iodide of potassium in the aggravation of the phenomena of poisoning. Five grains and four-tenths (0·350 gramme) of corrosive sublimate were given to a little dog in the following manner:

First day (0·150 gramme), $2\frac{3}{10}$ths grains.
Two following days, no poison.
Fourth day (0·050 gramme), rather more than $\frac{7}{10}$ths of a grain.
Four successive days, no poison.
Ninth day (0·050 gramme), rather more than $\frac{7}{10}$ths of a grain.
Tenth day, no poison.
Eleventh day (0·100 gramme), one and a half grain.

After this the dog was let alone. He continued to live, although very ill; he might, perhaps, have recovered. On the 19th day, that is to say, eight days after his last dose of sublimate, 61 grains (four grammes) of iodide of potassium destroyed life in twenty four hours. We have already seen that this dose of iodide of potassium may be given with impunity to healthy dogs. Its highly energetic action in the present instance cannot be understood, unless we admit that the sublimate had

become fixed in the body, and that the iodide of potassium, by giving it a new form, by rendering it soluble, and thus allowing it to be carried through the system, had developed an agent capable of exerting a poisonous effect on the organism.

I have not been able to pursue farther these experiments with the compounds of mercury. They deserve, nevertheless, to be kept in view, in connexion with the cure of patients affected with mercurial trembling.

§ 21. *Observations on the treatment of secondary syphilis.*

The last experiment reported finds a parallel in the treatment of the secondary and tertiary disorders of syphilis. The administration of iodide of potassium often causes intense suffering in patients who have been treated by mercurials. To what are these phenomena to be ascribed? According to the opinions put forth in this essay, it is at once seen that when, in consecutive disorders of this class, iodide of potassium is given to individuals who have been treated by mercurial compounds, two distinct effects are produced by a single agent; first, the compounds of mercury fixed in the body are rendered soluble and active; and secondly, a form is given to them which allows their (rapid) elimination. But by the very fact, the patient is subjected anew to a mercurial treatment by the compounds of mercury already present in the body.

If in the treatment of secondary syphilis iodide of potassium acts on its own account—a fact which I do not dispute—we must not, nevertheless, leave out of view the properties to which I have here desired to draw attention, for they must, without doubt, play an important part in the case. There ought to be a marked difference between the action of iodide of potassium on an individual free from mercurial compounds, and on one who holds mercurial compounds in his tissues. It results from the facts established in this memoir, taken as a whole,—whether they concern protection from poisoning, the aggravation of it even unto death, or, finally, the cure of those already poisoned,—that with the treatment by iodide of potassium the cure is never obtained except by a preliminary poisoning—poisoning which the physician has completely the power to regulate according to the strength of the patient. It suffices for this to begin by administering the iodide in a small dose, as M. Guillot and I proposed as long as six years ago. For a man, it is well to begin with fifteen grains (one gramme) a day, increasing the dose if the patient bear it well. It seems to me that inferences of some importance to medicine may be drawn from the facts contained in this memoir. Indeed, the constant aggravation of the symptoms of poisoning upon the administration of iodide of potassium in excess, and the elimination of the poison in a state of combination with one of the elements of the iodide, authorize us in saying that certain medicines act in the first instance on their own account; but may act at the same time by the agency of the materials they meet with in the living body. It is the duty of the physician, therefore, to inquire into the prior history of the patient, even more perhaps than physicians are in the habit of doing, when he is desirous of administering remedies having an action analogous to that of iodide of potassium to individuals formerly subjected to the influence of saturnine or mercurial compounds.

Art. II.

On certain Elastic Structures connected with the deep flexor tendons of the Fingers and Toes. By John Marshall, F.R.C.S.E., Assistant Surgeon to University College Hospital.

Whilst engaged some time since in a careful dissection of the human hand and foot, I found, in connexion with the tendons of the flexor profundus muscle, both of the fingers and toes, certain slender bands of yellow elastic tissue, of which I do not remember to have met with any description. Having, by a sufficient number of independent observations, satisfied myself of the constant presence, in the parts above mentioned, of these small elastic ligaments, I have endeavoured to ascertain in what way they are modified by the several conditions of age, sex, occupation, and disease; and, moreover, so far as my limited opportunities have permitted, I have studied the varieties they present in the limbs of different vertebrate animals. The object of the present communication is to put on record the facts thus accumulated, and to advance some general considerations on the use and import of these somewhat interesting, though, in man at least, apparently not very important structures.

A. *Human Anatomy.*—It is well known to the human anatomist, that the tendons both of the superficial and deep flexor muscles of the fingers and toes, are provided, where they lie in their common sheaths along the phalanges, with certain delicate cords or folds, which pass off from their deep surface, and connect them loosely with the phalanges beneath. These little ligamentous folds did not escape the notice of some of the earlier anatomists, but it appears that they were first more especially examined, delineated and named by Weitbrecht, whose account of them has been either adopted or confirmed in the descriptions of all subsequent anatomical writers.

a. *In the Hand.*—By Weitbrecht (Syndesmologia, 1742), those present in the hand are classed with other similar bands at the wrist and on the backs of the fingers, under the title, *Vincula tendinum accessoria;* those opposite to the phalanges being separately described as situated *circa phalanges digitorum* (op. cit., sect. ij. p. 51). Two sets are pointed out in the hand, as connected with each of the tendons of the flexor sublimis and flexor profundus—viz., the *ligamenta vel vincula brevia et longa.*

The *long* ligaments (*tenacula superiora seu gracili. Auctor.*) consist of one, *ligamentum longum perforati,* which passes forward from the first phalanx to the tendon of the flexor sublimis vel perforatus; and of *two* others, *ligamentula longa perforantis,* which commence, one from a membrane joining the split portions of the perforated tendon, and the other from the first phalanx, and are attached to a slight median longitudinal ridge (*crenula*), found on the deep surface of the tendon of the flexor profundus. These ligamentula longa, says Weitbrecht, are not constant, but vary in number and arrangement. They are very slender, and of a filamentous nature, and are generally so long, that even in the utmost extension of the finger, they do not seem able to be stretched. Nor, from their extreme fineness, can we suppose any traction to be exercised through them. Hence he adds, "I dare not determine their use. But, however slender they may

be, all of them are seen after injection to be covered with elegant arteries and veins."

The *short* ligaments (*tenacula inferiora seu lata;* Auctor.) are two in number, one for each of the two associated flexor tendons. The *ligamentum breve perforati,* according to Weitbrecht, arises from the *palmar* surface of the first phalanx, opposite the depression found just above the anterior articular extremity; it has a rhomboidal figure, and is attached to the tendon as far as its insertion. The *ligamentum breve perforantis,** which is seen when the deep flexor tendon is slightly raised, "has the form of a triangle (the *triangular frænum;* Theile), the apex of which extends to the insertion of the tendon, whilst the opposite border or base is free and concave. Both of the ligamenta brevia consist of a doubled membrane, and often contain fat within them."

Now, on reviewing this description, which is, for the most part, literally translated from Weitbrecht, it is necessary to remark, in reference to the long ligaments (see fig. 1, *a, b b,*) that besides acting as vincula, they undoubtedly serve to convey vessels, and probably nerves, to or from their respective tendons. In regard to the short ligaments, it is likewise certain that they help to keep the tendons in place, and support nutrient vessels; but something more has to be said concerning them. In the first place, the ligamentum breve of the flexor sublimis or perforatus (fig. 1, *c*), is connected not merely with the palmar surface of the first phalanx, but with the palmar or glenoid ligament of the articulation between the first and second phalanges, and also with the synovial capsule of that joint. Moreover, so far as I have observed, it does not extend to the insertion of the perforated tendon, but its lower border is free as well as the upper.† Lastly, this vinculum is short, and non-elastic; and its direction, which is slightly downwards and forwards during the extension of the finger, is changed to upwards and forwards, whenever the finger is strongly flexed. From all which, it appears to me that this ligament performs another mechanical, though probably subsidiary, office, during flexion of the finger—viz., that of drawing upward, and, therefore, away from the action of the joint, the glenoid ligament and especially the loose synovial capsule situated just above it. So, too, the ligamentum breve of the flexor profundus vel perforans (fig. 1, *d*) is connected, by many fibres, with the synovial capsule and glenoid ligament of the articulation between the second and last phalanges, as well as with the palmar surface of the second phalanx, and hence it exerts a like action on these parts, during flexion of the finger, to that just named in regard to the other short ligament.

Occasionally, but only as an exception, I have observed that the apex of the ligamentum breve perforantis does not quite reach forward to the insertion of the deep flexor tendon, but leaves an opening there beneath the tendon. Generally, this condition was met with in the forefinger only, but once it existed in the fore and middle fingers of both hands. Very frequently I have found a Pacinian body, or two, seated deeply in the apex of one of these ligaments. But the fact to which I wish particularly to direct attention concerning them, and which distinguishes them from all the other vincula accessoria, is, that they contain within their duplicature

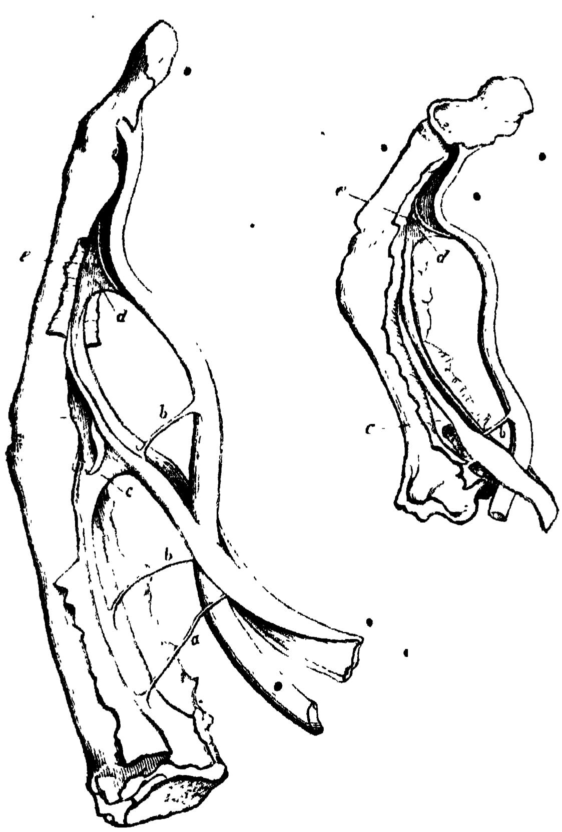
* Weitbrecht, Tab. v., figs. 17 & 18, *k*.

† Upper and lower mean proximal and distal; anterior and posterior mean palmar and dorsal.

the *small elastic ligaments,* to which allusion has been made at the commencement of this paper, and which, as they seem to have hitherto escaped notice, it is my special purpose here to describe.

In a characteristic specimen, let us suppose in the middle finger (fig. 1), on gently pulling the deep flexor tendon upward and forward from the bones, the elastic ligament is recognisable without any dissection, embedded

Fig. 1. Fig. 2.



Figs. 1 & 2. Lateral view of middle finger, and of second toe, with their two tendons, and respective vincula accessoria. *a* Ligamentum longum perforati. *b b*, Lig. longa perforantis. *c*. Lig. breve perforati. *d*. Lig. breve perforantis. *e*. Vinculum subflavum.

in the triangular vinculum (*d*), and appearing on either side as a fine yellowish streak (*e*), running obliquely from the under side of the tendon downwards and backwards in the direction of the last joint of the finger. If, after the tendon has been thus drawn away from the bones, it be now let go again, the yellow streak will be seen to retract or shorten itself, and at the same time to form a very slight ridge on the sides of the triangular

vinculum. From its peculiar colour, its distinctness of outline, its special direction, and its remarkable elasticity, this yellow band is easily distinguishable from other whitish and less defined streaks, lying also within the triangular vinculum, which pass directly backwards, or even slightly upwards from the back of the tendon to the second phalanx, and which consist of bands of white filamentous tissue, or of blood-vessels. It is also quite distinct from the interposed bundles of fat. If the flexor tendon be now carefully severed near its insertion into the last phalanx, without injury to the triangular vinculum, or its contained parts, the fact of some elastic structure existing within the vinculum may be made still more decidedly evident, by alternately elongating it, and allowing it to retract. At the same time, it may be thus more readily ascertained that the elasticity resides in the yellow band above alluded to; and, moreover, it may now be seen, that this band itself is attached on the one hand, to the flexor tendon, and on the other, to the second phalanx, in the manner to be immediately described.

The *vincula subflava*, as I would venture to name these small elastic ligaments, may be dissected either from the side, by removing one layer of the duplicature of the synovial membrane of which the triangular vinculum is composed (as shown in fig. 3, *e*), or from their superficial aspect, as

Fig. 3.

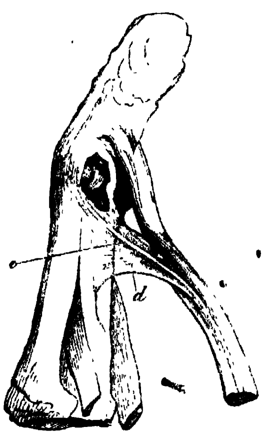
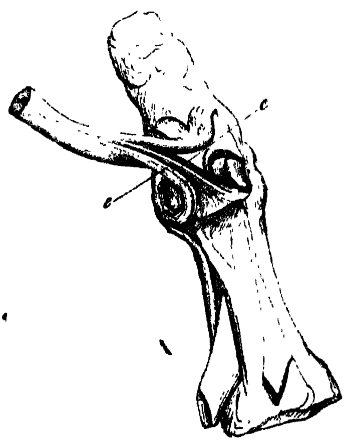


Fig. 4.



Figs. 3 & 4. Lateral and under or deep view of vinculum subflavum (*e e*), as dissected, to show its attachments and relation to the synovial membrane and articular surface of the second phalanx. In all the figures the vincula are shown as if much stretched.

when the flexor tendon is cut across close to its insertion, or from their deep surface (as shown in fig. 4, *e e*). In this way, each of them, when existing in the most characteristic form, is found to consist of a thin flat sheet of yellow elastic tissue, placed with its surfaces directed forwards and backwards, one towards the deep surface of the flexor tendon, the other towards the second phalanx, and having its borders or margins projecting slightly, as it were, on the lateral surfaces of the triangular vin-

culum in which it lies. This sheet or plane is narrow or pointed above where it is fixed to the tendon, and broader below, at its attachment to the bone; and it must be observed, that along the middle line, especially at its fore part, it is so thin, that only a few scattered bundles of elastic tissue are recognisable under a good magnifying glass, whilst, on the contrary, this tissue is accumulated in greater quantity along either border. Hence, each vinculum subflavum, in the human subject, appears rather to consist of two small rounded bundles of elastic tissue placed side by side in the same plane, and slightly diverging from each other as they pass downwards from the flexor tendon to the bone. This form will again be referred to, under the comparative anatomy of the part.

The lower, broader, and more fixed extremity of the elastic vinculum is attached to the front of the second phalanx, along a line, closely corresponding to the anterior margin of its lower articular surface, just outside and above the synovial capsule of the last finger-joint. The stouter lateral bundles take origin especially from the neighbourhood of the little bony eminences found at each angle of this articular border. The upper pointed and more moveable extremity of the vinculum subflavum reaches the deep surface of the flexor tendon, just opposite to the lower third of the second phalanx, and is attached along each side of the slight median ridge seen upon the under surface of the tendon. At each extremity, the lateral bundles are slightly spread out, so as to be less compact and defined than in the middle of their course. They are accompanied by many pale and loose bundles of filamentous tissue, which pass from the tendon to the glenoid ligament and synovial pouch of the last joint of the finger, or to the body of the second phalanx.

When completely retracted, the vinculum subflavum of the middle finger, in the adult, is nearly three-eighths of an inch in length; but when elongated as far as the ordinary action of the deep flexor tendon would occasion, it measures about three-fourths of an inch. Its widest part is about a quarter of an inch, and its thickness at the border not so much as one-thirtieth of an inch.

Examined under the microscope, both before and after the addition of acetic acid, the substance of the vincula subflava is found to consist of coalesced plates and fibres, possessing, in a marked degree, all the characters of the yellow elastic tissue.

Such being the condition of the vincula subflava, when most highly developed in the human finger, it is necessary to remark that one continually finds them in various stages of degradation; though, in almost all cases which I have hitherto examined, excepting in disease, I can truly say that a distinct representative of them exists, and in the few instances of the extremest grade of deficiency, scattered elastic bundles are still found, in their usual place. It may be well here, however, to particularize some of the varieties met with, and the conditions under which they occur.

In the first place, the dimensions of the vinculum subflavum correspond generally with the size of the finger: thus, as a rule, it is largest and thickest in the middle finger, next in size in the ring finger, then in the forefinger, and smallest in the little finger. It is generally less easily seen in the forefinger than in the ring or even in the little finger; but this depends partly on its being more obscured by the surrounding filamentous

tissue, which is proportionally denser in the triangular vinculum of the forefinger. When this triangular vinculum, in any finger, extends for an unusual distance up the second phalanx and the flexor tendon, I think I have observed that the elastic ligament within it is, for the most part, smaller than usual. On the contrary, when the former has a less extensive attachment, and thus admits of a greater degree of movement, and especially when it is perforated at its apex, then the elastic vinculum is very well marked.

Nearly always one edge or border of each vinculum subflavum is thicker and more prominent than another. In the middle, ring, and little fingers, it is generally the radial border which is more developed and apparent; whereas in the forefinger it is more frequently the ulnar border.

In the *thumb*, the triangular vinculum, as noticed by Weitbrecht, is delicate and long; and I am inclined to pronounce that it possesses no true vinculum subflavum resembling those of the fingers. For all that I have been ever able to find, and that only seldom, is a single, long, slender, semi-transparent cord, passing from the long flexor tendon to the middle of the first phalanx, and not to the neighbourhood of its lower articular extremity. In the section on the comparative anatomy this will again be noticed.

Between the *two hands* I have observed no material difference, except that the vincula subflava, though probably not larger, are, from the greater delicacy of the triangular vinculum, more evident in the *left* than in the *right* hand.

In the *female* hand they are certainly quite as marked, and, in relation to the smaller dimensions of the hand, quite as large, as in the *male*.

In *youth* they are even more easily seen than at subsequent periods of life; but they exist in aged subjects. The influence of peculiar manual occupations, whether involving dexterity or force, I am not in a position to determine from sufficiently numerous observations; but, so far as I can judge, it certainly appears to me that hard labour by no means develops these elastic vincula, which I have usually found more distinct in more delicately formed hands. On the whole, from this circumstance, and from their being, if anything, relatively larger in youth, and having an inverse ratio to the extent of the triangular vinculum, it would seem that freedom of movement in each finger, and not combined exertion, is the condition most favourable to their development.

They are, of course, more distinct, and are certainly more compact, in thin than in fat persons. In deaths from chronic disease, as phthisis, they are as evident as in those from sudden causes. I have had no opportunity of examining them, either in paralysis or in congenital deformity of the hand.

b. *In the Foot.*—The vincula accessoria of the flexor tendons of the toes, which are also well described by Weitbrecht (sect. v., p. 199), are essentially the same as those of the fingers; but they are necessarily smaller and shorter, in proportion to the smaller size of all the parts. The ligamentum breve perforantis, or triangular vinculum (fig. 2, *d*), extends up the phalanx a considerable distance, and contains within its layers the elastic ligament, which appears, when well marked, as a yellowish streak (*e*), on one or both sides. The vincula subflava of the foot are absolutely smaller

and shorter than those of the hand, but in proportion to the length and size of the toes they are quite as long and as wide. Their microscopic characters are quite as distinct; they present, however, greater occasional marks of degradation. They are larger in the second toe, and gradually diminish down to the little toe, in which there is often only a small quantity of representative elastic tissue. The tibial border of each vinculum subflavum is the one most usually developed beyond the other. In the great toe, as in the thumb, I have occasionally seen a very long and feeble elastic cord, which passed from the flexor tendon only to the middle of the first phalanx. As in the hand, neither age nor chronic disease produce much effect on the vincula subflava.

In a case of unremedied talipes varus, in a subject certainly seventy years old, the elastic vincula are present, though small. So, also, in a case of talipes equinus, in a body about thirty years of age, they are perfectly distinct—perhaps, even somewhat thicker than usual.

c. *Development.*— In a fœtus at about the third month, it was quite possible to distinguish the superficial and deep flexor tendons of the fingers or toes. At about the fifth month this can be done easily, though they are slightly adherent to each other, and to their common sheath. In a fœtus, born prematurely at the eighth month, the seat of the vincula subflava in the fingers and toes is occupied by a wide, dense mass of tissue, arranged in a linear manner, between the deep tendon and the second phalanx; but it does not possess much elasticity. This tissue resists the action of acetic acid, and appears under the microscope as if composed of clear-looking nuclear fibres. At birth, the yellow colour, defined outline, and special elasticity of the vincula subflava are quite evident; indeed, they seem proportionally thicker, and form a more continuous sheet, or plane, than in after life.

B. *Comparative Anatomy.*—The observations which I have hitherto made on the vincula subflava in the vertebrate animals, impart to the anatomy of these structures an interest which it would not possess, had the inquiry been limited to the human body. But the details of this comparative research, however novel and interesting, being too numerous for record here, I must restrict myself to little more than general statements.

The "vincula tendinum accessoria" of the fore and hind limb, in the vertebrata generally, remind one, in their arrangement, of those in the hand and foot of man; the exceptions being in such cases, for example, as the turtle, where the flexor tendons are blended opposite the phalanges with the surrounding parts; or as the mole, bat, dolphin, or reptilia, in which these tendons are altered in arrangement, small, or wanting. In all cases where the deep flexor tendons run independently to the last phalanges, there is a distinct synovial fold, or vinculum, situated beneath and near its termination, corresponding with the ligamentum breve perforantis in the human digit; but this is always less evident in the thumb and great toe, where they exist, than in the four outer toes or fingers.

It is within these terminal vincula *only*, excepting in the feet of birds, as will be presently explained, that the *vincula subflava* are to be found. They exist *in all vertebrate animals*, so far as I have yet examined, *which possess distinct perforating tendons* for the last phalanges of the fingers and

toes. As a rule, they are not met with either in the thumb or great toe, when those parts are present; one exception occurs in monkeys, where a small roundish cord exists in both those digits; but it must be observed, that in the quadrumana the flexors of the thumb and great toe are parts of the flexor perforans. In birds an elastic vinculum is present in the hallux or so-called great toe; but this, it may be remembered, is more probably the analogue of the second toe of mammalia.

In nearly all cases the vincula subflava are larger, in proportion to the size of the limbs, in animals than in man. In certain peculiar modifications of the limbs only, which entail a restricted movement of the individual phalanges—as in the seal, for swimming; or in the foot of the kangaroo, for leaping,—are these structures proportionally smaller than in the human body. In the mole, and in the wings of bats and birds, I do not find them. In all these cases the muscles are peculiarly modified.

They are very large and wide in the monkeys, more slender in form, but well marked, in the cat, and less distinct in the dog; they are strong in the hog, the ox, and the horse; well marked in the sheep and the hare, and weaker in the rabbit. They are very slender, or obscure, or defective, in the kangaroo and seal. In the feet of birds they are proportionally more highly developed than in any other animal, not only as regards size, but in certain respects, to be immediately mentioned. They exist in the crocodile and iguana; but not in the tortoise, turtle, salamander, or frog.

For the most part there is but one vinculum subflavum for each digit. But in many birds, perhaps in most, there is always found one in the hallux or inner toe; a very strong one, consisting of two planes, in the three succeeding toes; and, besides this, a supernumerary vinculum in the outermost, or two outer toes. This last circumstance cannot fail to be associated in the mind with the unusual number of phalanges, which are always four and five, in these toes. Quite as an exception to the rule in the vertebrata generally, there is sometimes found in the largest toe of birds an elastic vinculum, proceeding from the tendon of the perforatus, near the point of its splitting, to the second of the four phalanges of that toe. I may take this opportunity of mentioning, that Meckel* describes the deep flexor tendons of the toes in birds as ending "by slips, which are given off in succession to the ultimate and penultimate phalanges;" but the peculiarity of structure of the slip to the latter does not appear to have been noticed by him, or by any other observer; nor can I find that the vincula subflava have hitherto been described in any animal by comparative anatomists. These elastic structures having escaped notice in the human body, the key to their detection in animals was wanting.

In form, the vincula subflava, in the vertebrata generally, vary between two types—viz., that of a simple undivided sheet, and the bifurcated form. Thus, in monkeys and in certain birds, each vinculum forms a short wide continuous sheet, having thicker rounded margins and a thinner centre; the end attached to the bone being always broader than the one connected with the tendon. In most cases, however, as in the cat, dog, seal, hog, ox, sheep, horse, hare, rabbit, and kangaroo, and in some birds, they are bifid, the two slips of elastic tissue diverging from each other as they pass from the tendon. The bifid vincula are generally longer and more slender than

* System der Vergleichenden Anatomie, Band iii. p. 387.

the undivided ones; as, for example, in the cat, seal, and kangaroo, and some small birds, but not always, for they are short and thick in the hog, ox, sheep, horse, and ostrich. In mammalia, one border of the simple vinculum, or one slip of the bifid structure, is very frequently thicker than the other,—the preponderance generally being in favour of the radial and tibial sides. In the ruminants it is the median border which is the larger in each toe. In birds, I have found no difference in this respect: in these creatures, however, as already mentioned, the vinculum often consists of two planes, one broad, continuous, or bifid, the other of scattered bands of elastic tissue, placed immediately anterior to the other, connected with it closely by cellular tissue, and attached to the glenoid and capsular ligament of the last joint of the phalanges. The supernumerary vinculum of the two outer toes in birds, is long, single, and round in form, and is attached to the second and third of the four and five phalanges found in those toes.

Excepting in the instance of this supernumerary structure in birds, the vincula subflava are always attached to the penultimate phalanx, just above the synovial capsule, along the margin of the articular surface; their thicker borders or lateral slips, as the case may be, being there fixed to the two slight eminences at the inner and outer angles of the articular border; in this respect, as in so many points, reminding us of, and elucidating, the human structure. Hence, too, in extinct animals we may conceive the existence and character of the vincula subflava to be determined. In the cat, the outermost slip of the bifid vinculum is attached to the second phalanx, close to the insertion of the posterior elastic ligament, described by Sir Charles Bell, as serving to retract the claw in the feline tribe; but the one is perfectly distinct from the other.

In animals, contrary to what is found in man, but quite in harmony with the general superior development of the hinder extremity, the vincula subflava are usually more marked in that limb. In monkeys, however, there is no proportional difference between the hind and fore limbs. In the seal and kangaroo, I find them more evident in the anterior limb. No difference is observable between the right and left limbs. The effects of sex and age have not been noted; nor has the development of these vincula been traced. In the adult animal, microscopic examination invariably displays the characteristic appearances of the yellow elastic tissue.

C. *Uses.*—From the almost universal presence of these elastic vincula, and from their greater size in animals, as compared with man, it is obvious that no correct conclusions as to their peculiar office can be arrived at, without taking the widest view of their structure and arrangements.

Strictly mechanical in their office, they are probably most exactly adapted in each case to the requirements of the limb in which they are found, and are thus perhaps exempt from those general morphological laws which regulate the occasional degradation of so many of the special organs of the animal economy. Otherwise, these vincula, when small, as in man, might have been regarded as merely effete representatives or homologues of a prevailing type of structure. This view, however, can scarcely be correct, although their habitual small size, and occasional deficiency, sufficiently prove that in the human body their action must be comparatively subordinate. In the human foot, the movements of the last phalanges

are limited; and even the superiority of the hand depends chiefly on the number and isolation of its muscles and tendons, and on the comparatively equable balance between all its volitional movements.

The vincula subflava, considered generally, must act either on the penultimate phalanges, or on the deep flexor tendons, or on both.

a. *Action on the Bones.*—This could only be as subsidiary flexors of the penultimate phalanges, to which the deep flexor muscles ought then to be described as being in part attached. But independently, first, of the disadvantageous direction of the vincula subflava for such a purpose, inasmuch as they are inserted only into the distal end of the bones, instead of near their proximal end, as real tendons always are,—and, secondly, of their total unfitness for the economical transmission of muscular power, owing to their invariably elastic character,—it is evident, that in the majority of cases, including that of man, they are too weak to have more than a very insignificant effect on the flexion of the penultimate phalanges. Besides, the last phalanx must always be more or less flexed before the elastic ligament is drawn upon serviceably at all, in which case the penultimate phalanx must, as is well known, also be already flexed. In monkeys, and in digitigrade animals, in which they are generally well developed, these vincula may help slightly to flex the penultimate phalanges; and in birds, especially, they may serve to render steady, and yet as it were full of spring, the hold or grasp of all the orders of phalanges, during prehension, or while resting on the feet, or in the rapid movements of cursorial progression.

b. *Action on the deep flexor tendons.*—This appears to be the special office of the vincula subflava, and will be best apprehended by reflecting that their elongation and shortening in the natural action of the parts depend on the movements of the last phalanges upon the penultimate bones, and on no other movement of the digits. Thus, in moderate or extreme extension of the last phalanges upon the penultimate bones, the vincula subflava are at rest, their two points of attachment being then nearer together than in any other position of the parts. As soon, however, as the deep flexor muscle contracts, these two points begin to be drawn apart from each other, and the elastic ligaments begin to be stretched. At extreme flexion, they are elongated to the utmost. Hence, whilst the last phalanges are extended, and the deep flexor muscle is quiet, the vincula subflava are idle; but no sooner does flexion begin, than, by that very act, their elasticity is brought out, and they are ready, on the slightest relaxation of the flexor muscle, to draw forward or straighten out its several tendons through the bifurcation of the perforated tendons, and along the sheaths, in which they glide. In this way the vincula instantaneously and smoothly restore to the position of rest that long part of the flexor tendons which is included between the muscular belly and the points of attachment of the elastic vincula themselves. But at the same time—and this is also important—that part of the tendons situated beyond the vincula, between them and the ultimate attachment of each tendon to the last phalanx, is relaxed and put loosely at ease; and, accordingly, indirect but most efficient aid is thus given to the common extensor of these phalanges; a muscle which is comparatively feeble in power, which operates, in the first moment of its action, at a great disadvantage, and which would, without the ever-ready intervention of the elastic vincula, acting as they

do in a straight line, and without any expense of volition, have to draw out the long flexor tendons from their canals and sheaths, by voluntary effort, with extreme loss from friction, and with no small expenditure of misapplied power.

In the human hand, and particularly in the foot, this second mode of action of the vincula subflava must be very slight; but in most animals it is unequivocal; and in birds, in which the flexor tendons are very long and complex, and the latitude and rapidity of movement from extension and flexion very great, we find the vincula in their highest state of development, both as to number and strength. This special adaptation of the yellow elastic tissue, so distantly imitated by modern art in the vulcanized caoutchouc, for purposes which it so admirably fulfils, seems to me to be as striking and beautiful as any which have been previously pointed out in the mechanism of the animal frame, and the wide presence of the vincula in vertebrate creatures, vindicates their character for interest and importance. I am conscious, however, that many more observations are necessary to elucidate the varied degrees of power exercised by these vincula —a title to which they truly have every claim, in preference to that of tendon or ligament. Their study is, after all, but part of a larger subject —namely, the comparative and morphological anatomy of the muscles, and their accessories in the vertebrata.

Art. III.

On the Functions of the Muscles of the Tympanum in the Human Ear. By Joseph Toynbee, F.R.S., Fellow of the Royal College of Surgeons in England, Aural Surgeon to St. Mary's Hospital; Consulting Aural Surgeon to the Asylum for the Deaf and Dumb; and Consulting Surgeon to the St. George's and St. James's General Dispensary.

1. *The Articulation of the Stapes.*—The Stapes is generally described by anatomists as being connected with the margin of the fenestra ovalis by a simple membrane. Sir Anthony Carlisle, in his paper on the Physiology of the Stapes, merely speaks of "a membrane which connects it to the edges of the fenestra vestibuli."* Professors Sharpey and Quain agree with Sir Anthony Carlisle. They say, "The annular ligament of the stapes (ligamentum orbiculare vel annulare baseos stapedis) connects the base to the margins of the foramen fenestra ovalis. The fibres of the ligament are covered on the outer side by the mucous lining of the tympanum, and on the inner side by the membrane of the vestibule."† Mr. Wharton Jones describes this ligament as springing "from the margin of the vestibular fenestra, and it is inserted into the jutting margin of the base of the stapes all round."‡ Sœmmering seems to have had a different view respecting this articulation. He says, "A thin articular capsule connects the base of the stapes to the fenestra ovalis."§

If the circumference of the base of the stapes be carefully examined by

* Philosophical Transactions, p. 201. 1805. † Elementary Anatomy, p. 910. 1848.

‡ Cyclopædia of Anatomy and Physiology, vol. ii. p 548.

§ De Corporis Humani Fabrica, tomus secundus, de Ligamentis Ossium, p. 10. Huschke states that "Sœmmering was wrong in regarding this ligament as a capsule;" Huschke speaks of the *ligamentum annulare* only. The quotations from Huschke have been made since the paper was completed. See Encyclopædia Anatomique, par T. L. G. Bischoff, J. Henle, E. Huschke, &c., traduit de l'Allemand, par S. J. L. Jourdan. Tome v. p. 773. 1845.

means of a lens magnifying between three and four diameters, it will be apparent that instead of a fine margin only, it presents a distinct surface, which when *in situ* looks towards the border of the fenestra ovalis, and is separated from the inner and outer faces of the base by well-defined margins. This circumferential surface of the base of the stapes varies in breadth at different parts. The broadest part looks backwards, measures about a third of a line at its centre, and gradually narrows as it becomes continuous with the superior and inferior surfaces. This broad part, instead of looking directly backwards, is oblique, the direction of its surface being obliquely backwards and outwards. The *anterior* extremity of the circumferential surface of the base is not so broad as the posterior; and instead of being oblique, it is somewhat rounded. The upper and lower surfaces of the base of the stapes are narrower than the anterior and posterior portions; their middle part is the narrowest. (Fig. 1.) When examined in a recent ear, the circumferential surface of the base of the stapes is found to be quite smooth, and covered by a very delicate layer of cartilage, which communicates a soft sensation to the finger when it is touched by a fine probe. The cartilage is most abundant at the two extremities, from which portions of sufficient magnitude can often be removed, especially in young persons, so as to admit of their being examined by means of the microscope. It consists of oval corpuscles, very similar to those in ordinary articular cartilage, but considerably smaller.† The surface of the fenestra ovalis, to which the circumference of the base of the stapes is applied, is larger than that of the stapes. (Fig. 3.) The posterior surface does not quite correspond in its direction with that of the stapes; it looks directly forwards, instead of obliquely inwards and forwards, to face the stapes, which, it will be remembered, at this part, looks obliquely backwards and outwards. The articulating surface of the fenestra ovalis is smooth, and has a very compact appearance; no cartilage is detected upon it. It is bounded by two well-defined ridges. The circumference of the base of the stapes is attached to that of the fenestra ovalis by means of two membranes or ligaments. The inner or vestibular ligament passes from the inner margin of the fenestra ovalis to the inner margin of the circumference of the base of the stapes. The outer one passes from the outer margin of the fenestra ovalis to the corresponding margin of the base of the stapes. These two ligaments have between them a space which may be called the articular cavity; this cavity contains a sufficient quantity of fluid to lubricate the articulating surfaces of the bones. (Fig. 4.) By the action of the tensor tympani muscle, the base of the stapes is pressed inwards towards the vestibule as a piston in its cylinder; as soon as the tensor tympani muscle ceases to act, the ligaments above described being elastic, draw it outwards again.

Fig. 1.*

Fig. 3.

Fig. 4.‡

* The stapes from the right human ear. The circumferential surface of the base of the stapes is well seen in the subjoined drawing from the porpoise. (Fig. 2.)

Fig. 2.

† Huschke, speaking of the bones of the ear, says—"Their articular surfaces are encrusted with cartilage, which is covered by synovial membrane." He does not appear, however, to look upon the circumference of the base of the stapes as an articular surface, inasmuch as he describes a ligament as attached to it.

‡ A diagram of a horizontal section through the base of the stapes and the fenestra ovalis, showing the ligaments and the articular cavity.

2. *The Movements of the Stapes.*—The stapes is moved by two muscles, the *tensor tympani,* and the *stapedius.* Anatomists appear to agree, that the action of the *tensor tympani* is to press the stapes directly inwards towards the cavity of the vestibule; the general opinion appears to be, that the *stapedius* muscle merely assists the tensor tympani. Thus Mr. Wharton Jones says, "The first action of this muscle (the stapedius) will be to press the posterior part of the base of the stapes against the vestibular fenestra. At the same time the long branch of the incus will be drawn backwards and inwards, and the head of the malleus being, by this movement of the incus, pressed forwards and outwards, its handle will be carried inwards, and the membrana tympani thus put on the stretch. Breschet calls the muscle of the stapes a *luxator*, but I do not know on what grounds."* Professors Todd and Bowman write, "In contraction it (the stapedius muscle) would fix the stapes by pulling its neck backwards. It probably compresses the contents of the vestibule."† Ellis states that "it assists in retaining the stapes applied to the fenestra ovalis."‡ Muller writes, "The influence of the stapedius muscle in hearing is unknown .... The only effect which it appears to me could be ascribed to it, would be to render tense the membrane by which the base of the stapes is connected with the margin of the fenestra."§

On account of the smallness of the stapedius muscle, and the very slight degree of movement which it produces, it is difficult to determine in what way the stapedius muscle influences the contents of the vestibule. As the tendon of the stapedius muscle in its course forwards, passes slightly upwards, there is every reason to infer that it draws the neck of the stapes backwards and slightly downwards, and that it produces a slight rotation of the base. That this rotatory movement of the stapes has the effect of slightly withdrawing its base from the cavity of the vestibule is, I think, shown by the following experiment. The tympani cavity and stapedius muscle being exposed, the stapes is to be left in situ. By means of a small pair of cutting forceps a section is to be made through the cochlea, a portion of which should be allowed to remain connected with the vestibule. The scala vestibuli of this portion will be observed to be filled with fluid as far as the margin of the section, which fluid is of course continuous with the perilymph in the cavity of the vestibule. If the stapedius muscle be now pulled, or if the neck of the stapes be moved slightly backwards, the fluid in the exposed part of the scala vestibuli will be found to recede slightly into the scala vestibuli, and its surface to become concave; as soon as the stapes is allowed to return to its quiescent state, the fluid again passes into, fills the scala vestibuli, and assumes a rounded surface. Independently of this action on the contents of the vestibule, the stapedius muscle produces a slight relaxation of the membrana tympani. This is effected by the neck of stapes, in the act of rotation, passing outwards as well as backwards, whereby it presses slightly outwards the inferior extremity of the incus, while the body of the latter bone passes inwards, carrying with it the head of the malleus, thus necessarily causing the long process of the latter bone and the membrana tympani to pass outwards. It would therefore appear that the stapedius muscle is

* Cyclopædia of Anatomy, vol. ii. p. 549. † Physiological Anatomy, 1847, part iii, p. 71.
‡ Demonstrations of Anatomy, p. 286. § Elements of Physiology, by Baly, vol. ii. p. 1264. 1842.

the direct antagonist of the tensor tympani muscle, the former relaxing the labyrinthine fluid, the membrana fenestræ rotundæ, and the membrana tympani, and the latter rendering tense the labyrinthine fluid and the two membranes. This view is supported by the fact that the stapedius muscle is supplied by a branch from the portio dura nerve, and the tensor tympani by a branch from the otic ganglion.* It may therefore, I think, be fairly inferred that the function of the tensor tympani muscle is to protect the membrani tympani and the labyrinth from injury during loud sounds, while the stapedius muscle places these structures in a position to be impressed by the most delicate vibrations; and it would appear to be brought into action during the process of listening. Instances are not uncommon in which these two muscles are not able to act promptly, and the unpleasant consequences are manifest. Thus the loud noise produced by firing a cannon near to a person without any expectation of it on his part, before the tensor tympani muscle has time to contract, is often followed by the sensation of singing or buzzing in the ears, produced, most probably, by a concussion of the expansion of the auditory nerve; these sensations often endure during many years. Cases are not unfrequently met with in which the mucous membrane of the tympanum is thickened, and a considerable amount of dulness of hearing is the consequence; many patients thus affected hear sounds—the human voice, for instance—perfectly well when they are listening, but as soon as the act of volition is suspended, the same voice in the same position is not perceived. In these cases it would appear as if the action of the stapedius muscle were requisite to counteract the pressure upon the stapes by the thick mucous membrane. The friends of young persons suffering in this manner often imagine that there is no dulness of hearing, but merely a want of attention; the fact being, that the power of hearing certain sounds exists in these patients only during the exercise of an effort of the will, instead of being involuntary.

The tensor tympani muscle appears to be of use, not only to prevent the membrana tympani and labyrinth from being injured by powerful sonorous vibrations, but also to protect these organs from the forcible pressure of air, or of a foreign body. Thus the membrana tympani offers considerable resistance to the pressure of a foreign substance which is introduced into the meatus slowly; but the sudden and unexpected contact

* Since writing the above description my attention has been drawn to a monograph on the ear, by Huschke, above alluded to, in which he has arrived at similar conclusions respecting the functions of the stapedius muscle to those here advanced. As it is evident, from the quotations of writers on the ear, made above, that these views have not been entertained, I have not scrupled to publish my researches at length, although I withdrew them from a paper on the Physiology of the Tympanum and Eustachian Tube, which has been read before the Royal Society. The following are Huschke's words:—"While it (the stapedius muscle) presses the posterior extremity of the base of the stapes upon the posterior part of the border of the fenestra ovalis, it lifts the anterior extremity of this bone, and covers the fenestra. At the same time, the descending branch of the incus, with the stapes, is drawn backwards, by which the body of this bone presses the malleus forwards, and as its handle rests upon the membrana tympani it relaxes it. I have often observed this movement of the malleus when I moved the long branch of the incus in the direction of the tendon of the muscle of the stapes; I thus regard the latter as relaxing the tympanum and opening the labyrinth; that is to say, according to the view of Treviranus, it is the antagonist of the tensor tympani muscle. The two have altogether much analogy; they describe an arch looking upwards, pass over a kind of pulley, and are contained in an osseous canal; but they have also opposite functions; the stapedius muscle passes from behind forwards; the tensor tympani from before backwards; the stapedius receives its nerve from the facial, the tensor tympani from the fifth."*

* Encyclopædia Anatomique, tome v. pp. 782, 783.

of a similar body often produces extensive laceration of it. Again, a violent blow on the ear with the palm of the hand rarely produces mischief to the membrana tympani when its reception *is expected*, whereas, a comparatively gentle blow, when not expected, frequently produces not merely a concussion of the nervous labyrinth and very serious derangement of its functions, but the membrana tympani itself is not uncommonly ruptured.*

The preceding observations indicate that one function, at least, of the vesicles and muscles of the tympanum and the membrana tympani is to act as the analogue of the iris in the eye, and to regulate the amount of sonorous undulations that are to pass to the labyrinth. This view has already been to a certain extent alluded to by previous writers. M. Savart, in the course of his very interesting researches upon the functions of the membrana tympani, arrived at a somewhat similar opinion, although he did not point out the manner in which the muscles acted on the labyrinth and membrana tympani. He says, "Les osselets ont encore pour fonction de modifier l'amplitude des excursions des parties vibrantes des organes contenus dans le labyrinthe."† Mr. C. Brooke, in a lecture delivered at the Royal Institution, in the year 1843, says, "This osseous arrangement may be considered to perform an office in the ear analogous to that of the iris in regard to light,—namely, that of regulating the tension of the various structures that are thrown into a state of vibration, according to the pitch and intensity of the sound to be transmitted to the sentient nervous fibres. This was effected by the conjoined action of the tensor tympani and stapedius muscles, by which the tympanum would be rendered more tense, and a simultaneous change in the position of the stapes would alter the tension of the fluid throughout the labyrinth, and therefore also the tension of the membrane of the fenestra rotunda which intervenes between that fluid and the air in the tympanic cavity."‡ Professors Todd and Bowman state that there is "much reason to suppose that the tensor tympani muscle is analogous in its use to the iris, and destined to protect the organ from too strong impressions."§

The first effect of the destruction of the membrana tympani gives weight to the opinion here advocated. Mr. Busk lately detailed to me the particulars of a case in which, for a few days after the destruction of the membrana tympani, a patient was unable to endure the whistling of a patient in an adjoining bed; and Cheselden says, that after destroying the tympanum in both ears of a dog, "for some time it received strong sounds with great horror."‖

* I may mention two cases illustrative of the above statement. The *first*, of an eminent physician in London; while playing with his children, he received a blow on one ear from the head of one of them suddenly and rapidly coming into contact with it; from that time (at least four years since) to the present, there has been a constant singing in the ear. The *second* case is of a young gentleman, now under my care, who, in play, received a box on the ear from his tutor, who came silently behind him, this was followed by a slight bleeding and some pain: on examination, I observed a lacerated orifice in the membrana tympani.

† Recherches sur les Usages de la Membrane du Tympan et de l'Oreille Externe, par M. Felix Savart. Lu à l'Académie Royale des Sciences, le 29 Avril, 1822. Journal de Physiologie, par F. Majendie. Tome iv. p. 183.

‡ Lancet, 1843, p. 380.

§ Physiological Anatomy, part iii. p. 91.

‖ The Anatomy of the Human Body, fifth edition, 1740, p. 305.

Art. IV.

On the Specific Gravity of the Brain. By W. H. O. Sankey, M.D. Londin., Resident Medical Officer of the London Fever Hospital.

The observations on the specific gravity of the brain, the analysis of which forms the subject of the following pages, were commenced in December, 1846, and have been continued up to the present time.

The subjects of the observations were all patients admitted into the London Fever Hospital, under the care of the physicians, Dr. Tweedie and Dr. Southwood Smith.

All the notes used in the sequel were made at the date of the observations they record; those in the symptoms were written by the bed-side of the patient; and the morbid appearances discovered after death, and the results of the examination of the specific gravity, were committed to paper during the progress of the autopsy.

The notes of the symptoms were made by various observers; the larger number of them, by Dr. Jenner or myself; in some instances, by Dr. G. Birkett and Mr. Humphry. The descriptions of the pathological changes in the viscera have, in many instances, especial value, from the circumstance that they were made by Dr. Jenner, Professor of Pathology in University College, London, to whom also I am indebted for much important assistance afforded me through every stage of this investigation. I am also indebted to the same friends, above mentioned, for assistance in many of the autopsies. All the patients, whose cases are used, were seen repeatedly by myself, and I have taken part in all the post-mortem examinations. The observations on the specific gravity were taken by myself, and for their accuracy I am alone responsible.

The mode employed for taking the observations on the specific gravity requires a brief description. The apparatus used consisted simply of a number of tall glasses, each filled with a fluid of a different, but of a known density; the glasses being arranged in a line, and placed according to the density of their contents. The fluid made use of in the present observations was a solution of common salt. The number of glasses required varies according to the viscus which is to be examined; for taking the specific gravity of all the viscera, about twenty-four glasses are necessary; but for the brain alone fourteen or fifteen are sufficient.

The fluid in the different glasses was adjusted in my apparatus only to every alternate numeral of the same decimal; for example, for testing the brain, the fluid in the first glass was adjusted to the specific gravity of 1·050, that in the second glass to 1·048, in the third to 1·046, and so on to the specific gravity of 1·024. The most convenient method of adjusting the fluids to the density required, and maintaining them constantly at the same degree, is to keep always in each glass two of the specific gravity bubbles, which are sold by the barometer makers. Into the fluid of the density 1·050, for example, the bubbles marked 1·051 and 1·049 are placed. So long as the fluid continues at the density of 1·050, the bubble marked 1·051 will float, and that marked 1·049 will sink in it, and should any change take place in the density of the fluid, the bubbles will indicate the alteration by an alteration in their position; but as long as the bubble

marked 1·051 floats, and that marked 1·049 sinks, the fluid will be sufficiently near the mean density of the two—that is, 1·050—for all practical purposes.

The mode of taking an observation with the above apparatus is as follows:—The glasses having been arranged according to the density of the fluid they contain, and the bubbles in their proper position, one being at the surface of the fluid in each glass, and the other at the bottom of the glass, to test the density of the white matter of the brain, a small piece is cut from the centre of one of the hemispheres of the brain, and entirely separated from any of the grey substance. Small portions are then dropped from a height of one or two inches above the glass into the fluid, say of the density of 1·041; should it float in this fluid, a second piece is to be dropped into the glass containing the fluid of the specific gravity of 1·039; should it float in this also, a third piece must be dropped into the fluid of the density of 1·037, and so on until the fluid is arrived at in which it will sink. If, for instance, a portion of the white matter floated in all the fluids tried, at and above the density of 1·039, but sank in the fluid of the density of 1·037, its own specific gravity would be 1·038, or of a density the mean, or sufficiently near the mean, of the two fluids.

To avoid error, the operation should be repeated two or three times, and on the white and grey substance of each hemisphere separately.

The sources of error in making an observation on specific gravity by this mode are the same as those which pertain to all other modes—viz., the adhesion of bubbles of air to the piece under examination, or to the bubbles used, &c., &c.; but besides these, there are others peculiar to the method, which are due to the use of the salt. Most of the tissues of the body will imbibe common salt, and thus after a short time will sink in the fluid in which they at first floated.

This error was avoided by noting the first effect that followed the immersion of the tissue under examination; but since the present observations were made, I have endeavoured to find a fluid which would be free from the objection that pertains to a solution of salt; and I am inclined to believe, from experiments instituted, that a solution of sugar will be found entirely free from the evil complained of.

ANALYSIS.

GREY MATTER.

§ I. The specific gravity of the grey matter of the brain was ascertained in 73 subjects; the lowest density was 1·028, the highest 1·046: the mean of all the cases 1·0346.

§ II. Of the 73 brains examined, the density of the grey matter in rather more than a quarter (or 17) was 1·032; and in rather more than one-fifth (or 15), 1·034. In nearly half it varied between 1·032 and 1·034; and in two-thirds between 1·032 and 1·036.

WHITE MATTER.

§ III. The specific gravity of the white matter was ascertained in 75 cases; its mean density was 1·0412. The highest density noted was 1·048, the lowest 1·032.

§ IV. In nearly one-fourth the specific gravity of the white matter was 1·042. In about as many it was 1·040, in rather less than an eighth, 1·038, and in the same proportion of the whole, 1·041 and 1·044. In the next largest number of cases it was 1·043.

§ V. So that in the bulk, or six sevenths of the whole, the density of the white matter varied between 1·038 and 1·044.

(A.)

Influence of Sex.

GREY MATTER.

Sp. Gr. 1·0	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	Total
Males ...	1	...	1	..	10	3	7	2	..	...	3	2	...	1	5	1	...	..	...	36
Females	1	1	1	1	7	1	8	1	9	...	...	3	2	1	...	...	...	...	1	37
Total ...	2	1	2	1	17	4	15	3	9	...	3	5	2	2	5	1	...	...	1	73

§ VI. *Males.*—The specific gravity of the grey matter was ascertained in 36 males. The average of all the observations is 1·0353.

§ VII. *Females.*—The grey matter was tested in 37 females. The mean of the observations is 1·0349.

§ VIII. The means show, therefore, no material difference between the sexes with regard to the density of the grey matter.

§ IX. Or by another mode of comparison the specific gravity of the grey matter was 1·032 in 10 of the 36 males, and in 7 of the 37 females: it was 1·034 in 7 of the males, and 8 of the females. But the density most frequently met with among the males was 1·032, while that most often observed among the females was 1·036.

§ X. So that while the mean density of the grey matter is slightly higher in males than in females, the second mode of analysis tends rather to prove that the most frequent density in females is higher than that which is most frequent in males.

(B.)

WHITE MATTER.

1·0	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	Total
Males ..	1	..	...	1	...	...	5	1	7	5	9	1	5	..	3	...	. .	36
Females	. .	.	...	...	1	...	4	1	9	3	8	4	3	2	1	...	1	37
Total	1	...	...	1	1	..	9	2	16	8	17	5	8	2	4	..	1	75

§ XI. The mean specific gravity of the white matter, in males, as deduced from 38 observations, is 1·0410, and in females, from 37 observations, it is 1·0414.

§ XII. A very marked correspondence, also, exists in the variations. By a reference to table B it will be seen that in the columns in which the greater number of cases are placed, as in those denoting the densities 1·038, 1·040, and 1·042, the number of cases are, respectively, 4 males, 5 females; 7 males, 9 females; 9 males, 8 females.

§ XIII. It follows, therefore, so far as the number of my observations is capable of showing, that there is a great similarity in the density of the white substance in the two sexes.

(C.)

Influence of Age, and Age and Sex.

GREY MATTER.

Ages.	Males.		Females.		Both Sexes.	
	No. of Obs.	Mean Sp. Gr.	No. of Obs.	Mean Sp. Gr.	Total.	Mean Sp. Gr.
Under 15	3	1·0336	2	1·0340	5	1·0337
15 to 20	4	1·037	11	1·0353	15	1·0357
20 to 30	3	1·037	6	1·037	9	1·037
30 to 40	9	1·0355	4	1·0336	13	1·0349
40 to 50	9	1·0355	7	1·0358	16	1·0356
50 to 60	2	1·033	1	1·0290	3	1·0316
60 to 70	6	1·0365	4	1·0325	10	1·0349
70 to 80			2	1·0320	2	1·0320
Total ..	36	1·0354	37	1·0348	73	1·0346

§ XIV. Though the above table is not sufficiently extensive to give any accurate result, yet it appears to show, that the density of the grey matter is highest between the ages of 15 and 30 years in males, and 20 and 30 in females, or between 20 and 30, sex not considered.

§ XV. Or, by another method of examining the subject, it will be found that in 22 of the 36 males, the density of the grey matter was below the mean, i. e., 1·035 (§ vi.), and in 12 above it.

§ XVI. The average age of those, the grey matter of whose brain was below the mean, was 39 years. The age of those, the grey matter of whose brain was above the mean, 36 years.

§ XVII. By taking the same average for females, the actual density being 1·0349 (§ vii.), then there were 20 cases in which the density of the grey matter was below the mean, and 16 in which it was above it. The average age of the females in whom the density of the grey matter was below the mean, was 38 years, and of those in whom it was above the mean, 27 years.

§ XVIII. Again, in 5 cases, males and females, the density was below 1·031, and in 11, 1·040 and upwards. The mean age of those in which it was below 1·031, was 49 years. The mean age of those in which it was above 1·040, was 29 years. So that by this mode of analysis, also, it would appear that the density of the grey matter is greater in the early period of life than at advanced ages; and with regard to the influence of sex, it will be seen by reference to table A, that of the 11 observations in which the density of the grey matter was at or above 1·040, seven pertained to males, and four to females; and of the five in which it was below 1·031, two to males, and three to females.

(D.)—*Influence of Age, and Age and Sex combined.*

WHITE MATTER.

Ages.	Males.		Females.		Both Sexes.	
	No. of Obs.	Mean Sp. Gr.	No. of Obs.	Mean Sp. Gr.	No. of Obs.	Mean Sp. Gr.
Under 15	4	1·0380	2	1·0435	6	1·0398
15 to 20	5	1·0398	11	1·0405	16	1·0401
20 to 30	3	1·0416	6	1·0410	9	1·0412
30 to 40	9	1·0419	4	1·0400	13	1·0413
40 to 50	8	1·0418	7	1·0421	15	1·0419
50 to 60	3	1·0410	1	1·0420	4	1·0412
60 to 70	6	1·0413	4	1·041	10	1·0411
70			2	1·041	2	1·0410
Total......	38	1·041	37	1·0417	75	1·0413

§ XIX. By referring to the last column of the above table, it will be seen that the mean specific gravity of the white matter rises very slightly and regularly till the patients attained the age of 50 years, and then gradually falls. The same variations will be observed in the column devoted to specific gravity in the separate sexes; but the rise and the fall are much less regular in the column of the females than in that of the males. The variation, however, in all, is insignificant in extent.

§ XX. Or to examine the subject in the mode used with regard to the grey matter, and taking the mean of the white matter at 1·041 (§ xi.), then in 30 cases the specific gravity of the white matter was less than the mean—viz., in 15 males and 15 females, the average of the males being 31 years, of the females 30 years. In 37 the density was above the mean—viz., in 18 males and 19 females, the average age of the males being 38 years, that of the females 36 years.

§ XXI. This mode of examining the subject, therefore, leads to the same conclusion—viz., that there is very little variation in the density of the white matter at different ages, or in the different sexes.

I proceed to examine the effect on the specific gravity of the length of the period that elapsed between death and date of observation.

(E.)—*Influence of Post-Mortem Changes.*

No. of Observations.	Period of the examination. Hours.	Average hours at which the examination was made.	Mean Sp. Gr.	
			Grey.	White.
6	Under 12	8	1·0393	1·0416
26	24	23	1·0359	1·0414
9	36	29	1·0340	1·0405
19	48	44	1·0347	1·0417
9	72	68	1·0330	1·0417

Average hour after death at which all the observations were made, 34.

§ XXII. It appears from the above table, that the mean density of the grey matter is less the longer the post-mortem examination was deferred, while with the white matter no such effect is apparent.

§ XXIII. The difference between the first and second series, or between those examined at an average of eight hours, and those examined at an average of 23 hours after death, is much greater than, and bears no proportion to, the difference that is found between any two of the succeeding series. In attempting to account for this high degree of density observed in the first series, and the great difference that exists between it and the rest, the question naturally occurs, was there any peculiarity in the symptoms of these cases, that rendered an early examination of the brain advisable? Were they head cases? and was the autopsy hastened lest time should have effaced the traces of disease? This does not appear to have been the fact; for of the six patients, the post-mortem examination of whom was made thus early, two had severe cerebral symptoms, two slight delirium, and two died sensible. The actual specific gravity of the grey matter of the brain, in these six cases, was as follows:—of two it was 1·042, of one 1·041, of one 1·036, and of one 1·033; that the density of the grey matter would be found high, if the examination were made at a very early period after death, is not, therefore, universally true.

§ XXIV. In continuing the analysis of the rest of the series (and six cases are insufficient to draw conclusions from), it still appears that there is a pretty regular decrease of density, equal to about ·001 for every 24 hours that intervenes between the death and the autopsy. Is this due to changes in the brain after death? This question admits of being answered by a direct experiment. A portion of the brain of a man, killed by the bite of a cobra di capello, was examined 31 hours after death; the density of the grey matter was 1·039, of the white, 1·042. It was again examined 79 hours after death, when the grey matter had a density of 1·038, the white of 1·043. Again, seven days after death, the specific gravity of the grey matter was 1·036, of the white, 1·044. The portion of brain examined was simply wrapped in paper during the time that elapsed between the first and second examination, and it lay uncovered in the dead-house, in a plate, between the second and third experiments, the weather being cold and wet. At the time of the last examination, the outer portions of the grey matter were becoming slightly mouldy, and the white, at the thinnest parts, slightly dry and yellowish, or horny-looking.

§ XXV. The inference to be drawn from the above experiments appears to be, when the facts are closely examined, different from that which suggests itself on the first view. At first sight, these experiments appear to show that the lower densities of the grey matter in all my observations were mainly occasioned by the length of time that elapsed between death and the date of the autopsy, and that the greater densities met with were attributable to the early hour at which the post-mortem examination of the brain was made. The experiments do, indeed, show that the lapse of time, or its concomitant (i. e., probably decomposition), decreases the density of the grey matter. But they equally prove that the same influences, after acting under circumstances which might be supposed to be much more favourable to decomposition, as exposure to the air, &c., only reduced the grey matter, after the lapse of a week, to the density of 1·036.

Now, in none of the cases used in this analysis was the autopsy deferred beyond 72 hours, and the above reduced density of 1·036, is still much higher than the mean of all the cases.

§ XXVI. It will be seen that in the experiment just cited, the specific gravity, taken 79 hours after death, was just ·001, lower than that taken 31 hours post mortem; and this decrease will be found to be in exact accordance with the mean of density of the grey matter noted in all the series of table E, except that between the first and second period.

(F.)

Influence of Season.

No. of Observations.	Months in which the Observations were made.	Mean Sp. Gr. of Grey Matter.	Mean Sp Gr. of White Matter.	Average number of hours at which the Autopsy was made.
17	June, July, and August	1·0348	1·0401	28
22	November, December, January, & February	1·0350	1·0418	37

§ XXVII. The difference exhibited in the above, between the observations made in the summer and those in the winter, is ·0002. But with respect to the grey matter, since the average time after death at which the observation was made, was less in the summer by nine hours, the density should be greater by nearly ·0004, (§ xxiv.) After making this correction, the difference in the density of the grey matter in the summer and the winter months will be ·0002; the summer being the period at which the density was greater, the difference however, is trivial.

But although the interval which elapsed after death, before the examination was made, appears to exercise some influence on the density of the grey matter, yet it is obvious that the marked increase in those cases in which the density of the grey matter was found to exceed 1·040, was not wholly due to this cause; for out of the eleven observations in which the specific gravity of the grey matter was 1·040, or more (Table *A*),

4 were examined 12 hours after death,
2 „ 24 „
1 „ 36 „
3 „ 48 „
1 „ 50 „

While with respect to season or temperature,

1 was examined in January,
2 „ February,
3 „ March,
2 „ May,
2 „ June,
1 „ August.

§ XXVIII. From the above it is also evident that the first six cases in table E, in which the grey matter had an average density of 1·0393, and which were the only cases examined at so early a period after death, may have owed their greater density to other influences than that under consideration.

(G.)

Weight of the Brain in Adults, and the Specific Gravity of the White and Grey Matter.

	Sex.	Age.	Sp. Gr of Grey Matter.	Weight of Cerebrum.	Weight of Cerebellum.	Sp. Gr. of White Matter.
1	M.	38	1·032	33 ozs.	6 ozs.	1·041
2	F.	53	1·029	33½ ,,	5½ ,,	1·042
3	F.	65	1·034	34½ ,,	5½ ,,	1·042
4	M.	60	1·033	37¼ ,,	5 ,,	1·046
5	M.	65	1·034	38½ ,,	6¾ ,,	1·040
6	F.	20	1·036	39 ,,	5½ ,,	1·044
7	M.	55	1·034	39½ ,,	6½ ,,	1·040
8	F.	22	1·032	40 ,,	6¼ ,,	1·042
9	F.	65	1·028	40½ ,,	5½ ,,	1·042
10	F.	20	1·036	41 ,,	5½ ,,	1·043
11	M.	51	1·032	44 ,,	6¾ ,,	1·044
12	M.	26	1·035	46 ,,	6½ ,,	1·042
13	M.	25	1·034	48 ,,	7 ,,	1·041
14	M.	37	1·032	51 ,,	6½ ,,	1·044

The above table is arranged according to weight of the cerebrum, the lightest being placed first.

§ XXIX. The number of cases in adults, in which the weight of the cerebrum and cerebellum was taken, does not amount to more than fourteen, but from the above table there appears to be no correspondence or relation between the weight and the specific gravity of the brain; the heaviest brain neither has the lightest specific gravity, nor the contrary; nor has the lightest brain the lowest specific gravity of either white or grey matter.

(H.)

Influence of the Duration of the Last Illness on the Specific Gravity.

Died.	No. of Observations.	Mean Specific Gravity.		Autopsy: Hours after death.
		Grey Matter.	White Matter.	
Before 7th day	5	1·0412	1·0420	17
From 7th to 14th	21	1·0350	1·0414	37
,, 14th to 21st	14	1·0351	1·0416	27
,, 21st to 28th	6	1·0345	1·0431	38
,, 28th to 35th	4	1·0355	1·0407	45
,, 35th to 60th	4	1·0327	1·0406	41
Longer periods	5	1·0308	1·0404	37
Total	59	1·0349	1·0414	

§ XXX. It will be seen that there is a wide difference between the density of the grey matter in those cases which terminated before the seventh day and those that ended fatally after that period. There also appears in the above table a general tendency in the grey matter to decrease in density as the length of the last illness increases, but the decrease proceeds much less rapidly after the seventh day than it appears to do up to that date.

§ XXXI. The great difference between the density of the grey matter in the first and second lines of the above table (H), closely resembles the difference between the first and second lines of table E, in which the influences of post-mortem changes are recorded. It becomes a question, therefore, whether in table H the great density observed in the first line is due to the period at which the autopsy was performed. On examination this does not appear to be the cause, for although the average number of hours at which the brain was examined was considerably less in the first line of table H than in the rest of the series, yet it is too great to account for the excess of density over the mean; for the density, it will be seen, is even higher in table H than in table E, though the autopsy was performed later in table E than in table H.

§ XXXII. But if persons dying before the seventh day have the grey matter of the brain of a much higher density than those dying after a longer illness, it becomes a question whether this greater density is the normal condition of the grey matter, unaltered on account of the speedy termination of the disease; or, on the other hand, is the greater density a pathological change in the brain, and the cause, or at least co-existent with the cause, of the rapid death. If the former were wholly the case, we should expect that the symptoms during life, and the cause of death, would be referable to some other viscus than the brain. Or, on the latter supposition, we should naturally infer that grave cerebral symptoms would precede death.

§ XXXIII. The causes of death and the symptoms exhibit both conditions. The causes of death were pneumonia with granular kidneys and disease of the heart, in two cases: double pleurisy with effusion, and diseased liver and kidneys, in one; acute encephalitis in one; scarlatina in one.

Examined with respect to the head-symptoms exhibited during life.

The first case was noted to be "perfectly sensible and rational up to a few minutes prior to death;" the second, "utterly unconscious, with contracted pupils and convulsions;" the third, "slight delirium, sensible when roused; death by apnœa;" the fourth, "some talking while dozing; sensible when roused; the fifth, "delirium, and leaving the bed and hiding himself; answers correctly when roused."

One, therefore, was sensible, three slightly delirious, one had grave cerebral disturbance.

§ XXXIV. There remains, however, untouched the supposition that great differences in density of the grey and white substances of the brain may be found in different individuals, existing as the normal condition, and that the variations recorded are totally unconnected with any pathological influence whatever. This supposition will be more conveniently examined further on, when the analysis of the specific gravity, in connexion with the symptoms, comes to be considered. (Vide § liii.)

§ XXXV. The coincidence, however, on any supposition, is remarkable, that the grey matter of all the adults who died before the seventh day should have had the great density of 1·042, and further, that the fifth and only remaining case of those dying at the same early period should have had the density of the grey matter at 1·038, the subject of the case being a child of seven years of age, at which period of life the density of the grey matter appears to be normally somewhat lower than in adult age. (Table C, § xiv.) That this may be nothing more than a coincidence, the fewness of the observations renders highly possible; and in the next case, arranged according to the duration of the last illness, the grey matter had a density of 1·032. This case was that of a female, ætat. 63, dying sensible on the eighth day of an attack of pneumonia.

§ XXXVI. Direct experiment on the brain of an adult killed by accident, received while in perfect health, would be of much interest. I have not been able to procure such a case, but a portion of the brain of the man killed in two hours by the bite of a cobra, on October 20, was kindly furnished by the medical officers of University College Hospital; and on examination 31 hours after death, the specific gravity of the grey matter was found to be 1·039, the white 1·042. The man, it appeared, was under the influence of intoxicating liquors at the period of the accident, and could not be said to be in full possession of his mental faculties. But this observation rather favours the hypothesis, that in active health the density of the grey matter is usually high, and that length of illness is accompanied by a gradual diminution of the density of the grey matter. We shall perceive further on, that this decrease may take place and the mental faculties remain intact.

Specific Gravity of the Brain, examined in relation to the Cerebral Symptoms that existed during the Last Illness, and more especially during the Last Hours of Life.

§ XXXVII. *Classification.*—To classify the subjects of the observations according to the cerebral symptoms exhibited by them during life, is obviously a task of much difficulty; the degree of importance to be assigned to a single symptom, or to any collection of symptoms, is not sufficiently established to allow of distinct classes being readily made. In the following analysis, the cases have been separated from each other, and formed into four series, in the following manner:

In the first series are classed all those cases in which the patients were perfectly sensible up to the period of death.

In contrast to these, the fourth series is composed of all those cases in which the graver cerebral symptoms were well marked, such as convulsion, strabismus, contraction, paralysis, or utter unconsciousness, when independent of coma.

Between these extremes, a large number of cases exists, all with more or less delirium, and they have been subdivided into two, making the second and third series. In the second are placed those cases with the least severe cerebral disturbance, and which proved fatal, through some other than the nervous system, as when death took place by apnœa, exhaustion, &c. In the third series are arranged those cases in which death occurred through the nervous system, as indicated by coma, &c.

The first series is composed of 23 cases, in which death occurred from the following causes, the patients all dying sensible:—pneumonia, 6; peritonitis, 2; pericarditis, 1; bronchitis, 1; ascites and anasarca, 1; dysentery, 3; cancer, 2; sloughing of the back, 2; scrofulous disease of the hip, 1; traumatic erysipelas in a tubercular subject, 1; gangrena senilis, 1; sudden death, 2.

Of the last two, one had typhoid fever, and much frothy blood was found in the left ventricle of the heart; the other dropped down dead while apparently convalescent, and softening of the fornix was the only lesion discovered.

The second series comprises 16 cases of the following diseases:—typhus fever, 3; typhoid fever, 4; relapsing fever, 2; scarlet fever, 1; dysentery, 1; pneumonia, 2; pleurisy, 2; pyohæmia, 1.

The third series is composed of 25 cases—viz., typhus fever, 19 cases; typhoid fever, 4; dysentery, 1; tubercular pneumonia, 1.

The fourth series consists of 12 cases, of which two have been added since the other tables were made for this analysis, and one is also included in the first series.

The last case is that of a man, the notes of whose symptoms during life are less perfect than could be desired; the patient had had apparently a slight febrile attack on his first admission into the hospital, accompanied with much delirium. He had been marked convalescent for 14 days, when, while sitting by the fire, he was observed to alter in the features, and fall. He was placed on the bed, and died immediately. On examination of the body, after death, the viscera were all healthy, with the following exceptions:—A thick false membrane covered the anterior surface of the left lung, readily separable from the pleura, and the fornix was reduced to about the consistence of clotted cream. In the classification according to the symptoms, the case has been placed in the first series; but considering that death was attributable only to the pathological changes found in the brain, and that the history of the symptoms is imperfect, it has been thought just to place it in the fourth series also.

In all the cases, therefore, composing the fourth series, cerebral disease was present, as manifested either by the symptoms or by the lesions.

(I.)

The Specific Gravity of the Grey Matter examined in relation to the Symptoms immediately preceding Death.

GREY MATTER.

Series.	Mean duration of case in days.	Mean No of hours after death of autopsy.	No. of Obs	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	Mean	[illegible]
1	35	40	23	2	..	1	..	6	3	5	..	4	.	..	..	..	1	1	..	..	..	..	1·033[illegible]	[illegible]
2	17	28	16	..	..	1	1	1	..	4	2	2	..	2	..	..	1	2	..	..	..	..	1·0357	[illegible]
3	17	36	25	..	1	.	..	10	1	6	1	2	..	..	3	1	..	..	..	..	..	..	1·0352	41
4	16	27	12	..	..	..	..	..	..	..	..	1	..	1	2	1	1	2	2	1	..	1	1·041	25
		Total ..	76	2	1	2	1	17	4	15	3	9	..	3	5	2	3	5	2	1	.	1		

§ XXXVIII. The above table exhibits a much higher mean specific gravity of the grey matter in the fourth series, than in the other three; and in glancing at the individual items from which the mean is deduced, it is seen that all the numbers are arranged at that end of the table denoting the higher densities.

§ XXXIX. The mean density of the grey matter of the cases composing the fourth series of table I, is higher than the mean exhibited in any other table, with the exception of table H, which gives the influences of the duration of the last illness on the specific gravity. In table H, it was shown that the mean density of the grey matter of those who died before the seventh day is 1·0412. The mean duration of the last illness, however, in the cases composing the fourth series of table I, was 16 days, and it is seen (in table H) that the mean specific gravity of all the cases dying between the fourteenth and twenty-first day, is 1·0351, so that the great density of the grey matter exhibited in the fourth series of the table under consideration, is not due to the length of the last illness.

§ XL. The next circumstance which, from the foregoing tables, would appear to have any influence in increasing the density of the grey matter, is the period at which the autopsy was performed. In table E, it appears that in the cases in which the autopsy was performed at an average of eight hours after death, the mean density of the grey matter is 1·0393, while in those examined later the mean density is lower; but the cases composing the fourth series of table I. have even a greater mean density than 1·0393, but were examined at a later period—viz., at an average of 16 hours after death. So that the greater density of series 4 is not due to the period at which the autopsy was performed.

§ XLI. With the exception of five, all the cases in which the density of the grey matter exceeded 1·040, are in the fourth series. The exceptional cases were of the following diseases:

FIRST SERIES:

1. Sudden death.—Softened fornix.
2. Heart disease, pneumonia, granular disease of kidneys, and cirrhosis.

SECOND SERIES:

3. Pneumonia and granular kidneys.
4 Pleurisy with effusion—kidneys diseased.(?)
5. Granular disease of the kidneys—Meningeal apoplexy.

So that all the cases which I have collected for this analysis, and which exhibited a density of the grey matter exceeding 1·040, have been either cases of disease of some part of the encephalon, or of disease of the kidneys. The notes of the state of the kidneys in the case of pleurisy with effusion are imperfect; but it is stated in them that the capsules of the kidneys were not readily separable.

§ XLII. It becomes a matter of interest to know whether these two diseases bear a separate relation to the increased density observed, or whether, by bearing a relation to each other, they have therefore a relation to the high density of the grey matter of the brain. If this relation be one of sequence, it is clear that the disease of the kidneys is not consequent on the disease of the brain, both from what is already known of the diseases, and from the facts exhibited in the cases themselves; for in some

of the cases very little cerebral disturbance was ever manifested. If one disease be the sequel of the other, the disease of kidneys is doubtless the primary affection, and we ought to find, therefore, a diseased state of kidneys, or traces of it in all the cases. Or if, instead of one disease being secondary to the other, if a common cause acting through both occasion the high density of the grey matter of the brain, then we ought to find disease of the kidneys in all the cases of the fourth series, and some manifestation of disease of the brain in those cases of the second and third series which had a density of the grey matter above 1·040; but between which the only circumstance in common appears to have been some disease of the kidneys. On examining the twelve cases composing the fourth series, with regard to the state of the kidneys, it appears that there was found disease of the kidneys in three, in one of which the organ was distinctly granular. It would appear, therefore, that the two classes of diseases—viz., disease of the kidneys and disease of the encephalon are separate and distinct as regards their relation to the greater density of the grey matter which was found in connexion with each of them.

§ XLIII. With regard to the relation between a diseased state of the kidneys and a high specific gravity of the grey matter, from analysis of sixty-four cases, in which the notes are sufficiently full on the state of the kidney for the purpose, there were found twelve cases in which the kidneys were granular, and the mean specific gravity of the grey matter of the brain in these was 1·0371; and in fifteen other cases in which the kidneys were otherwise diseased, the mean specific gravity was 1·0346. And in thirty-eight cases in which the kidneys are stated in the notes to have been healthy, the mean specific gravity of the grey matter was 1·0350.

It would appear, therefore, that the mean density of the grey matter of the cases in which there was marked granular disease of the kidneys, is somewhat less than the mean density of the grey matter of the head-cases.

§ XLIV. It must not be lost sight of, however, that the mere increase of specific gravity of the grey matter may represent widely different states of that tissue as regards its ultimate structure; and of course the disease consists, not in the increased density, but in the change of structure on which the higher specific gravity depends. There is nothing marvellous, I think, in the circumstance, if it should prove to be correct, that two diseases, widely different in their symptoms or seat, should similarly affect the mere density of the same tissue. And so far as the number of my cases will permit of any inference being drawn from them, they would seem to show that the increased density of the grey matter which co-exists with disease of the kidneys may be quite unconnected with cerebral symptoms, while the cases in which the grey matter of the brain is above 1·040, and which are not connected with disease of the kidney, are accompanied by severe cerebral disturbance.

§ XLV. To continue the examination of table I. It appears that the mean specific gravity of the first series of cases, or of those who died sensible, is 1·033, or slightly lower than the mean of all the cases, and this low density is not accounted for by the age of the subjects of the observations, nor by the date at which the autopsy was made, which was only one hour later than the mean of all the rest. With respect, however, to the length of the last illness, which was thirty-five days, the mean density

of the series is higher than the mean of all the cases (as shown in Table H) which died at the same period of disease.

§ XLVI. With respect to the second and third series in the table, the specific gravity of the grey matter is slightly above the mean in both; but the difference is trivial; the mean density of the second series is rather less than the mean of the third series, though the cerebral symptoms were more severe in the latter than in the former; the difference is not greater, however, than can be accounted for by the difference of mean age of the cases.

§ XLVII. Lastly, with regard to the cases in which the grey matter was found much below the mean, there are two in which the density was 1·028, or taking the mean at 1·034, as much as ·006 below the average. The cases both occur in the first series, or among those in which the patients were sensible up to the time of death; both patients died after a protracted illness, one of dysentery, the other of anasarca; one a man of 40 years of age, the other a woman aged 65 years. The former was of remarkably low mental capacity, the other was a shrewd and intelligent person.

(K.)

The Specific Gravity of the White Matter of the Brain, examined in relation to the Symptoms immediately preceding Death.

WHITE MATTER.

Symptoms	Mean duration of case in days.	Mean No of hours of autopsy.	Mean Age.	No.	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	Mean.
Series 1	35	40	36	24	..	..		1	1	..	3	2	8	2	3	1	2	..	1	..	..	1·0403
„ 2	17	28	26	17	..	..	..	.	..	..	3	.	2	2	6	2	2	..	..	..	..	1·0412
„ 3	17	36	41	25	..	..	..	..	.	.	3	.	5	4	7	2	2	..	1	..	1	1·0416
„ 4	16	27	25	12	2	..	.	..	..	..	.	.	1	.	1	2	2	2	2	..	..	1·0418
		Total	...	78	2	..	..	1	1	..	9	2	16	8	17	7	8	2	4	.	1	1·0411

§ XLVIII. There is a great uniformity exhibited in the mean density of the white matter in the four series of table K. The mean specific gravity of the first series is, however, slightly lower than the rest, but the average length of last illness in these cases was greater, and sufficient to account for the difference. (*Vide* table H.)

§ XLIX. In examining the items, however, from which the means were calculated, there appear some few which call for especial notice. There are two cases in the fourth series remarkable on account of the lowness of their specific gravity, and which are the lowest observed in any series. These cases have a density lower by ·009 than the mean of all the cases; they form a marked contrast to the other items of the fourth series. It will be observed that all the items but the two in question are above the mean density of the white matter. If the exceptional cases were withdrawn from the series, the mean density of those remaining in the fourth series would be 1·0438. The cases with the low density occurred in

children of eight and ten years of age, and were both of tubercular meningitis, and the low density is not accounted for, either by age, sex, length of illness, or any influence examined in the preceding tables.

§ L. It would therefore seem that the low density in these cases was due to a pathological change in the white matter generally; and that such an effect on the density of the white matter may take place, is rendered probable by observations made on the density of the cerebral matter in cases of softening of the fornix, in which the softened tissue was found to have a density below that of the density of the white substance of the rest of the brain.

In one of the two cases in which the specific gravity of the white substance was 1·032, there was also found a pulpy state of the fornix, and the specific gravity of the softened tissue was only 1·022.

In another case, in the fourth series, in which the density of the white substance was 1·044, the fornix was softened, and of the density of 1·032. In a third case of the same series, the white substance generally was 1·040, and the softened fornix 1·024.

The fornix, however, from my observation, is normally of somewhat lower density than the white substance of the hemispheres, in the proportion of 1·0357 to 1·0411.

§ LI. There would appear to be two states of the tissue, therefore, in the cases composing the fourth series; one, in which the density is less than the mean, and the other, in which it is slightly greater than the mean; for having deducted the two cases, as before stated, having the specific gravity of 1·032, the rest of the series have a mean of 1·0438, and this result accords with what was found to be the effect produced on the density of the grey matter in the same series, and also, with what will be hereafter shown to occur in conjunction with severe lesions met with by the ordinary mode of examination of the brain.

§ LII. To pursue the examination of table K, two cases of remarkably high density are found in the third series. Both cases were of typhus fever; one a male, the other a female. Another case of high density also occurs in the first series, or among those in which no mental disturbance was exhibited. The case was one of pneumonia and granular kidneys in a man aged 60 years. In the cases in the third series of typhus, the kidneys were healthy in both.

Lastly, there are two cases in the first series in which the density was 1·028, or taking the mean at 1·034, ·006 below the average. The cases in question were those of an Irishman, æt. 40, who died of dysentery, after a long illness, and a female, 65 years of age, who died after lengthened suffering from anasarca and diseased liver and kidneys. (*Vide* § xlvii.)

§ LIII. In conclusion, it would appear, therefore, that when the density of the grey matter is greatly above the mean, disease in some part of the cerebrum is always present; but that there may be a density of the same tissue considerably below the mean without cerebral symptoms. While, on the other hand, a decidedly high or low density of the white matter seems to be always connected with obvious disease of the brain.

Column.		(A) Brain Healthy. No.	Grey S.	No.	White S.	(B) White Substance, Normal Consistence. No.	Grey.	No.	White.	(C) White Substance, Firm. No.	Grey.	No.	White.	(D) White Substance, Soft. No.	Grey.	No.	White.	Means. No.	Grey.	No.	White.	Column.
	Cerebrum:																					
1	Brain Healthy	12	1·0346	13	1·0404													12	1·0346	13	1·0404	1
2	White Substance, normal					9	1·0326	10	1·0409									9	1·0326	10	1·0409	2
3	" firm	7	1·0358	7	1·0412					25	1·0348	26	1·0418					32	1·035	33	1·0416	3
4	" soft													8	1·0353	8	1·0406	8	1·0353	8	1·0406	4
5	Fornix, softer than normal									2	1·032	—	1·040	1	1·028	1	1·038	3	1·0306	3	1·0333	5
6	" pulpy					0		1	1·032	3	1·0403	3	1·043	3	1·0433	2	1·0435	6	1·0418	6	1·0413	6
7	Septum Lucidum, soft													1	1·028	1	1·038	1	1·028	1	1·038	7
8	Corpora Striata, soft									1	1·032	1	1·040					1	1·032	1	1·040	8
9	Convolutions, packed					0		1	1·032	3	1·0413	3	1·0436					3	1·0413	4	1·0407	9
10	Gyri, large	1	1·028	1	1·042					2	1·033	2	1·041					3	1·0313	3	1·0413	10
11	Grey Matter, layers marked	1	1·034	1	1·040	2	1·0335	2	1·045	6	1·0356	6	1·0413					9	1·0349	9	1·0419	11
12	" " not marked					0		1	1·032	4	1·032	4	1·042					4	1·032	5	1·036	12
13	" soft									1	1·038	1	1·046					1	1·038	1	1·046	13
14	" pale									3	1·032	1	1·042					3	1·032	1	1·042	14
15	" dark	1	1·034	1	1·040	0		1	1·032	3	1·034	3	1·0423	1	1·043	0		5	1·0358	4	1·0439	15
16	" injected					1	1·034	1	1·044	3	1·0386	3	1·045	2	1·0405	1	1·040	6	1·0366	5	1·0438	16
17	Commissura Mollis, of good consistency									9	1·0364	9	1·0425					9	1·0364	9	1·0425	17
18	" not discovered									2	1·032	2	1·042	1	1·038	1	1·040	3	1·034	3	1·0413	18
19	Cerebellum, firm	1	1·035	1	1·038					2	1·033	2	1·040					3	1·0336	3	1·0393	19
20	" soft													2	1·0355	2	1·042	2	1·0355	2	1·042	20
21	" vascular									1	1·042	1	1·042					1	1·042	1	1·042	21
22	Vascularity, generally increased					3	1·032	3	1·041	4	1·038	4	1·0432	5	1·0336	5	1·0394	12	1·0346	12	1·0402	22
23	" " diminished					2	1·031	2	1·040					4	1·0372	4	1·0467	6	1·030	6	1·041	23
24	" bloody points numerous	1	1·036	1	1·043	4	1·0345	4	1·0412	10	1·0366	11	1·042	7	1·0391	6	1·0415	22	1·0369	22	1·0417	24
25	" " not numerous									4	1·0345	4	1·041					4	1·0345	4	1·041	25
26	Dura Mater, vascularity increased	1	1·033	1	1·038					6	1·0376	6	1·0435	4	1·039	3	1·0425	10	1·0381	9	1·0432	26
27	Pia Mater, vascularity increased	2	1·035	1	1·040	2	1·033	2	1·0415	11	1·0356	11	1·0414	9	1·0363	8	1·0413	24	1·0356	22	1·0413	27
28	" " not increased					1	1·034	1	1·0440	4	1·036	4	1·0425					5	1·0356	5	1·0428	28
29	Membranes, Arachnoid, opaque					1	1·032	1	1·038	3	1·0373	4	1·042	7	1·0364	6	1·0425	11	1·0362	11	1·0419	29
30	" " dry					0		1	1·032	1	1·042	1	1·042	1	1·043	0		2	1·0425	2	1·037	30
31	" strip easily	2	1·0310	2	1·041	4	1·0327	4	1·0442	9	1·0334	9	1·0425	6	1·039	5	1·042	21	1·0346	20	1·0426	31
32	" " with difficulty	2	1·0345	2	1·041	3	1·033	3	1·033	3	1·040	3	1·044	2	1·037	2	1·040	10	1·0363	10	1·0393	32
33	Serum under arachnoid	2	1·033	2	1·0395	4	1·033	4	1·0425	16	1·0349	16	1·0426	13	1·0369	12	1·0411	35	1·0353	34	1·0418	33
34	" in ventricles	6	1·0343	6	1·0393	3	1·033	4	1·0375	18	1·035	19	1·0406	14	1·0361	13	1·0408	41	1·0351	42	1·0395	34
35	" at base	2	1·036	2	1·0405					9	1·0342	9	1·0418	7	1·036	6	1·0418	18	1·0351	17	1·0416	35
36	Lymph in ventricles									1	1·042	1	1·044					1	1·042	1	1·044	36
37	" at base					4	1·0327	4	1·041	1	1·042	1	1·044	2	1·0445	1	1·044	7	1·0374	6	1·042	37
38	Pus in ventricles									2	1·041	2	1·0445					2	1·041	2	1·0445	38
39	" at base									1	1·042	1	1·044					1	1·042	1	1·044	39
40	Tubercle in pia mater					0		1	1·032	1	1·040	1	1·045	1	·032	1	1·041	2	1·036	3	1·0393	40

LIV. *Explanation.*—The foregoing table exhibits the specific gravity of the white and grey substance in relation to the morbid anatomy of the brain. It is divided into four. Under the division A are arranged all those cases in which the brain is noted healthy; under B are all the cases in which the brain was described to have the white substance of normal consistence; under C those cases in which the brain was noted to have the white substance firm; under D those in which the white substance was described to be soft; and the last column gives the averages of all the cases.

The list of morbid appearances in the first column is composed of descriptions extracted from the note-books.

The table is read in the following manner:

Line 11. Layers of the grey matter marked, of which morbid appearance one example occurred in a brain noted to be healthy, and the specific gravity of the grey matter was 1·034, of the white matter 1·040. Two also occurred in brains, the white substance of which was of firm consistence, the mean specific gravity of the grey matter was 1·0335, and of the white 1·0425. Six examples occurred in cases in which the brain was described to have the white matter firm, and the mean density of the six is, of the grey matter 1·0356, of the white 1·0413. Total number of brains found with the layers of the grey matter well marked, 9, the average density of the grey matter of all being 1·0335. Average density of the white matter in all the cases, 1·0419.

§ LV. The table exhibits a notable correspondence between high density of the grey matter and some of the most severe lesions of the brain. For example, a high mean density of the grey matter is found in connexion with the following morbid changes:

Dryness of the arachnoid; lymph in the ventricles; lymph at the base of the brain; vascularity of the cerebellum; pulpy state of the fornix; convolutions closely packed; pus found in the ventricles: increased vascularity of the dura mater; grey matter soft; lymph found at the base. All the above morbid states correspond to a density of the grey matter above 1·037. Those connected with the highest density are placed first, and the others follow in the order of their density.

On reversing the arrangement, by placing first those morbid appearances which are found co-existing with the lowest density of the grey matter.

Then the list becomes thus:

Septum lucidum, soft; vascularity of the whole encephalon generally diminished; fornix, softer than normal; gyri, large; corpus striatum, soft; layers of the grey matter, not distinct; grey matter, pale; white substance, soft. All the cases having the above morbid appearances had the mean density of the grey matter below 1·033, those lesions which were connected with the lowest mean density being placed first.

The same mode of examining the morbid changes in connexion with the density of the white substance exhibits similar results—namely, the graver morbid changes, especially those believed to depend upon inflammatory action, are found co-existing with greater density, while the changes more allied to the opposite condition of brain co exists with a lower density of the white matter.

The following lesions are arranged in the order of the density of the white substance of the brain, found in conjunction with them, those being placed first in which the mean density is the highest.

Pus in the ventricles; pus at the base of the brain; lymph in the ventricles; grey matter, dark; grey matter, injected. In all these the mean density of the white matter is above 1·043.

In the next list, the lesion in which the white substance had the lowest density is placed first.

Fornix, softer than normal; layers of grey matter not marked; arachnoid, dry. In all these the mean density of the white matter is below 1·037.

Summary.—The foregoing analyses of 77 observations made upon the specific gravity of the brain, render probable the following general conclusions—viz.

That the mean specific gravity of the grey matter, in either sex, is 1·034; that the density of the grey matter is somewhat below the mean in the earlier and later periods of life; that the highest density is met with between the ages of 15 and 30 years in males, and between 20 and 30 years in females; that the density of the grey matter is, in a slight degree, lower in those persons who have died after a long illness, and greater, to a slight extent, in those subjects examined before twelve hours after death than in those examined at later periods.

That the density of the grey matter may be found in a subject after death to be ·006 below the mean, without any cerebral symptoms having been present during life; but when the specific gravity exceeds the mean by ·006, then one of the following conditions has existed during life—viz., either acute cerebral disease, attended with head symptoms of the gravest character, or chronic disease, (in all the cases analyzed of chronic disease of the kidneys,) attended either with no cerebral symptoms, or only with slight delirium.

That the mean specific gravity of the white matter after death is 1·041; that its density varies less than that of the grey matter in the sexes, or in the different periods of life; that it is much less affected by post-mortem changes or length of the last illness.

That in those cases in which the gravest cerebral symptoms were present during life, the density of the white matter after death may present two opposite conditions—either it may exceed the average, or it may be much below the mean.

That high specific gravity of both grey and white matter is found in conjunction with those morbid conditions of the brain connected with hyperæmia, and that a low specific gravity exists in conjunction with the opposite condition of the brain.

That no relation appears to exist between the specific gravity and the actual weight of the brain.

Art. V.

The Action of Liquor Potassæ on the Urine, in Health. By E. A. Parkes M.D. Lond., Professor of Clinical Medicine in University College, and Physician to University College Hospital.

The following pages contain the record of some observations instituted to determine the effect of liquor potassæ on the urine of a healthy individual, preliminary to a similar inquiry into its action on the urine of diseased persons.

A series of experiments on the action of liquor potassæ on the serum of the blood, and a recognition of the strong affinity which this alkali has for sulphuric acid, led me to examine, in the first instance, the effect produced on the sulphuric acid of the urine. Before doing this, it was necessary to determine the normal amount of sulphuric acid in the urine of the person experimented upon, under different conditions, and the following table exhibits the results of this inquiry.

I.

Quantity of Water, Solids, and Sulphuric Acid in the Urine of a Healthy Man thirty-three years of age, on ordinary Mixed Diet.

Date, 1852.	Time when urine was secreted and passed.—Quantity.	Quantity per hour in round numbers.	Solids in 1000 parts.	Sulphuric acid in 1000 parts.
March 11 ...	From 12 P.M. to 8 A.M.	...	.	1·079
„ 12 ...	„	. .	.	1·647
„ 13 ...	„	..	.	1·371
„ 15 ..	„	. .	39·18	1·038
„ 16 ...	„	...	54·47	2·32
„ 17 ...	„	..	52·51	1·371
„ 18 ...	„	...	27·41	1·785
„ 19 ...	„	...	24·22	1·831
„ 22 ...	„	.	48·27	1·713
„ 23 .	„	...	59·39	2·196
„ 24 .	„	. .	41·03	1·467
„ 25	„	...	31·72	1·718
„ 26 ...	„	. .	34·21	1·459
April 4 . .	From 9 A.M. to 11 P.M. ℥xxxiij ...	℥ij ʒij.		
„ 4, 5 ...	From 11 P.M. to 4 A.M.	...	28·21	1·048
„ 5 ..	From 4 A.M. to 8 A.M	...	60·	2·103
„ 5, 6 ..	From 8 A.M. to 4 A.M., ℥xxix.. .	℥j ʒiiss.		
„ 6 . .	From 4 A.M. to 8 A.M., ℥ix.........	℥ij. ʒij.	30·83	1·306
„ 6, 7 . .	From 8 A.M. to 4 A.M., ℥xxv.	℥j. ʒij.		
„ 7 ...	From 4 A.M to 8 A.M., ℥xiij.	℥iij. ℥ij.	40·7	1·533
„ 13, 14 ...	From 11 P.M. to 8 A.M. ℥ix.	℥j.	60·2	1·841
„ 15 ..	At 8 A.M.	..	46·35	2·262
„ 15, 16	From 8 A.M. to 8 A.M., ℥xxxviij ...	℥j. ʒiv. ℈ij.	51·59	1·867
„ 15, 16 ...	From 11 P.M. to 8 A.M.	...	44·16	2·364
„ 20, 21 ...	From 12 P.M. to 8 A.M., ℥xvij. .	℥ij. ʒj.	47·68	1·472
	From 8 A.M. to 8 A.M., ℥xxxviij. .	℥j. ʒiv. ℈ij.	42·27	1·189
„ 21, 22 ..	From 12 P.M. to 8 A.M., ℥xj.	℥j. ʒiij	30·40	1·378
	From 8 A.M. to 8 A.M., ℥xl...... .	℥j. ʒv.	41·14	1·248
May 13, 14 ...	From 11 P.M. to 8 A.M., ℥xvij.... .	℥j. ʒvij.	23·15	·866
„ 14, 15 ...	From 12 P.M. to 8 A.M., ℥xvij ...	℥ij. ʒj.	23·23	1·079
	From 8 A.M. to 8 A.M., ℥xlv.	℥j. ʒvij.	28·85	1·058

Quantity of Water, Solids, and Sulphuric Acid, &c.—continued.

Date, 1852.	Time when urine was secreted and passed.—Quantity.	Quantity per hour in round numbers.	Solids* in 1000 parts.	Sulphuric acid in 1000 parts.
May 15, 16 ...	From 12 P.M. to 8 A.M., ℥xiij......	℥j. ʒv.	41·57	1·479
	From 8 A.M. to 8 A.M., ℥xxxvj. ...	℥iss	50·85	1·59
„ 17, 18 ...	From 11 P.M. to 8 A.M., ℥xiij.....	℥j. ʒiiiss.	47·65	1·842
	From 8 A.M. to 8 A.M., ℥xxxiij....	℥j. ʒiij.	43·03	1·271
„ 18, 19 ...	From 11 P.M. to 8 A.M., ℥xv......	℥j. ʒv.	30·01	·894
	From 8 A.M. to 8 A.M., ℥xlvj. ..	℥j. ʒvij.	32·29	·811
June 2, 3 ..	From 8 A.M. to 8 A.M., ℥xxxiij. ...	℥j. ʒiij.	34·91	1·081
„ 3 ..	From 1 A.M. to 8 A.M., ℥viij.	℥j.	33·21	1·942
„ 3, 4 ...	From 8 A.M. to 8 A.M., ℥xxxviij...	℥i. ʒiv. ℈ij.	48·21	·991
	From 12 P.M. to 8 A.M., ℥xivss. ...	℥j. ʒviss.	25·2	1·15
„ 4, 5 ..	From 12 P.M. to 8 A.M., ℥xvj	℥ij.	23·55	1·306
	From 8 A.M. to 8 A.M., ℥xlvj.	℥j. ʒvij.	30·53	1·012
July 12, 13 ...	From 12 P.M. to 8 A.M., ℥ix.	℥j. ʒj.	44·59	1·701
„ 20 ...	From ½ past 3 A.M., to ½ past 7 A.M., ℥viss..	℥j. ℥v.	40·93	·822
„ 21 ...	From 1 A.M. to 4 A.M., ℥ivss. ..	℥j. ʒj.	26·05	1·120
„ 22 ...	At 8 A.M.........	...	29·41	·555
„ 24 ..	From 1. A.M. to 8 A.M., ℥vij.	℥j.	41·64	1·972
„ 25, 26 ..	From 12 P.M. to 8 A.M. ℥xij.	℥j. ʒiv.	38 8	1·260
	From 8 A.M. to 8 A.M., ℥xxxij.... .	℥j. ʒij. ℈ij.	29 84	1·170
August 2, 3 ...	From 12 P.M. to 8 A.M., ℥xiij. .	℥j. ʒv.	42·58	1·72
„ 3, 4 ..	From 12 P.M. to 8 A.M., ℥xx.	℥ij. ʒiv.	23·17	1·263
	From 8 A.M. to 8 A.M., ℥xlviij......	℥ij.	27·92	1·176
„ 4, 5 ..	From 12 P.M. to 8 A.M., ℥xix... .	℥ij. ʒv.	22 13	1·187
	From 8 A.M. to 8 A.M., ℥xlviij....	℥ij.	19 62	·811
„ 5, 6 ...	From 12 P.M. to 8 A.M. ℥xiv......	℥j. ʒvj.	29·92	1·165
	From 8 A.M. to 8 A.M., ℥xliij	℥j. ʒvj.	31·16	1·197
„ 10, 11 ...	From 11 P.M. to 8 A.M., ℥xvj......	℥j. ʒvj.	27·2	·978
	From 8 A.M. to 8 A.M., ℥xxx... .	℥j. ʒij.	34 58	1·737
„ 11, 12	From 11 P.M. to 8 A.M., ℥xxiij. .	℥ij. ʒivss.	27·29	·75
	From 8 A.M. to 8 A.M., ℥xlviij. ...	℥ij.	19·83	·971

AVERAGES.

1. *Urine secreted during the night, and passed in the morning.*
   Average amount per hour (mean of 155 hours) . . . = ℥j. ʒvj. ℈ij.
   Average of solids in 1000 parts (38 observations) . . = 37·27
   Average of sulphuric acid in 1000 parts (42 observations) = 1·4608
2. *Urine secreted during 24 hours.*
   Average amount in 24 hours (18 days' observations) . = ℥39. ʒvij.
   Average per hour . . . . = ℥j. ʒv. ℈j.
   Average of solids in 1000 parts (16 observations) . . = 35·41
   Average of sulphuric acid (16 observations) . . . . = 1·199

It appears from this table that the amount of sulphuric acid bore no

* In determining the amount of the solids, a very small quantity (four to eight grains) of urine was used. The sulphuric acid was precipitated as sulphate of baryta, from an acidulated urine. It has been said, that by this method an alkaline or a carbonated ash is sometimes left after the washed sulphate has been heated to redness. In a great number of experiments, I have never found the sulphate either carbonated or alkaline. Occasionally, in a very dark urine, it carries with it a little colouring matter, but this can be avoided by dilution. A more serious inconvenience is, that some of the sulphate passes through the pores of the finest filtering paper. For this reason it is advisable not to use paper at all, but to wash the sulphate by repeated relays of water, which is afterwards drawn off with a pipette. I have only further to remark, that I am quite certain of the accuracy of the observations on the quantity of urine, as the experiments were performed on myself.

relation to the amount of solids. A very concentrated urine contained certainly more solids and more sulphuric acid than a very dilute urine; but beyond this general expression, no rule can be laid down. Sometimes the solids were in great, and the sulphuric acid in small quantity, as on the 3rd and 4th of June, when the solids were 48·21 per 1000, and the acid was only ·991. Sometimes the reverse held good, as on the 4th and 5th of June, when in the morning urine the solids were only 23·55, and the sulphuric acid was 1·306 per 1000. It is also evident that the relative quantities of water and of sulphuric acid in 1000 parts in twenty-four hours is equally varied.

From the facts recorded in the table, and from others presently to be mentioned, the influence of the following circumstances on the excretion of sulphuric acid can be more or less perfectly determined—viz., the time of year, the time of day, exercise, food, excess of liquid.*

1. *Time of year:*

In March.—Average of sulphuric acid in morning urine . . (13 observations)	=	1·684
In April.—Average of sulphuric acid in morning urine . . (9 observations)	=	1·700
„ Average of sulphuric acid in urine of 24 hours (3 observations)	=	1·434
In July.—Average of sulphuric acid in morning urine . . (6 observations)	=	1·24
„ Sulphuric acid in urine of 24 hours (1 observation)	=	1·170
In August.—Average of sulphuric acid in morning urine) . (6 observations)	=	1·177
„ Average of sulphuric acid in urine of 24 hours . (5 observations)	=	1·178

These observations appear at first sight to show that the amount of sulphuric acid in the urine was greater in the cold than in the hot months. The quantity passed in 24 hours is, however, almost the same. Thus, in the three observations in April, on the urine of 24 hours, the average flow of urine was 39 oz., and the average SO_3 in 24 hours, 26·78 grs. In the five experiments in August on the urine of 24 hours, the average flow was 44½ oz., and the average, SO_3 in 24 hours, was nearly 24 grs. The higher per centage in the cold months was therefore attributable in great measure, if not entirely, to an excess of water in the hot months, from some unexplained cause.

2. *Time of day:*

During the night the average amount both of water, of solids, and of sulphuric acid, was greater than in the urine of twenty-four hours, and *à fortiori* than in the urine of the day. The excess of water and of solids was inconsiderable; that of sulphuric acid much greater. The reason of the excess may perhaps be found in the fact, that the heaviest meals in the day were taken in the evening—viz., at six, seven, and eight o'clock; and it is well known that the solids, and among them the sulphuric acid, are increased after food. To determine whether the excretion of the soluble sulphates of the food, or the rapid oxidation of the sulphur of the surplus albuminous aliment, would account for the excess of sulphuric acid in the morning urine, the principal meal was taken on eight occasions at two p.m., and very little or no food was taken afterwards. The average amount of the sulphuric acid present in the urine secreted between twelve midnight and eight on the following mornings, was 1·196 per 1000, or precisely the average of the urine of twenty-four hours. In addition to this cause, it will not appear

* The influence of food and exercise had been previously determined by Dr. Bence Jones. *Vide* various vols. of the Philosophical Transactions. I have merely enumerated my similar facts in order to complete the record of the investigation.

improbable, from what will be said of the effect of exercise, that the muscular movements taken during the day may really cause an increased oxidation of sulphur during the night.

3. *Exercise.*

The amount of sulphuric acid was increased by exercise, and it was found that the effect lasted a considerable time. Thus on one occasion, two or three hours after long-continued violent exercise, the sulphuric acid was raised to about ·6 per 1000 over the average; fifteen hours after the termination of the exercise, and thirteen or fourteen hours after food, the sulphuric acid amounted still to 1·743 or ·544 over the average.

4. *Food:*

The increase in the amount of sulphuric acid noted after food was perceptible in two hours, and continued to increase for four or five or six hours, after which the amount commenced to fall towards the figure which represented the oxidized sulphur of the tissues. The increase appeared to be more marked after meat than after bread.* The effect of food and of violent exercise together, was to cause a great excretion of sulphuric acid. On one occasion this amounted to 3·342 parts in 1000, or three times the average amount.

5. *Excess of liquid:*

The experiments on this point were not sufficiently numerous. When from twenty to thirty ounces of weak tea were taken beyond what the system demanded, the experiments showed an inconsiderable augmentation of the sulphuric acid.

Besides these conditions, there must be others affecting the oxidation of the tissues, as is proved by the extraordinary variation in the amount of sulphuric acid excreted, although the amount of exercise, and of solid and liquid food, did not apparently equally vary. The chemical agencies in the body no doubt alter continually, though the exact formula of the variations cannot of course be given. When long periods are taken, these temporary influences neutralize each other and disappear.

The above being, as far as I could determine, the variations in the state of health, the following table (II.) shows the effect of liquor potassæ on the

Water, Solids, Sulphuric Acid, and Acidity.

Date, 1852.	Meals—time and kind.	Whole amount of liquid taken during the period of experiment	Liq. potassæ, quantity of, and time when taken.	Time when urine was secreted and passed. Quantity.	Quantity per hour.	Reaction.	Solids per 1000 parts.	Sulphuric acid per 1000 parts.
March 26	Dinner at 6—soup, meat, bread, vegetables, water.	Not noted	At 11 P.M., liq. potassæ (P.L.), ʒss. in ℥ij of distilled water	From 11 to 9 A.M. (27), ℥x.	ʒj.	Acid.	42·083	1·798
„ 28	Dinner at 2—meat, vegetables, water. At 7—bread, meat, and tea.	„	...	At 11 P.M., 3¼ hours after food	..	..	58·61	1·618
„ 28, 29			At 11 P.M., liq. potassæ ʒj., and ℥ij of distilled water.	From 11 to 3, ℥v.	ʒj. ℈ij.	Acid.	55·56	2,327
„ 29	.. ..			From 3 A.M., to ½ past 7 A.M., ℥iv. ʒvj.	ʒj.	Acid.	57·907	2·406
„ 30	Dinner at 6—meat, vegetables, bread, water.	„						

* Dr Bence Jones's experiments are opposed to this statement.

Water, Solids, Sulphuric Acid, and Acidity—continued.

Date, 1852.	Meals—time and kind.	Whole amount of liquid taken during the period of experiment.	Liq potassæ; quantity of, and time when taken.	Time when urine was secreted and passed. Quantity.	Quantity per hour.	Reaction.	Solids per 1000 parts.	Sulphuric acid per 1000 parts.
Mar. 29, 30	... ...	"	..	From 11 P.M. to 9 A.M., ℥x.	℥j.	Acid.	51·67	2·475
" 30	Dinner at ½ past 5—meat, bread, vegetables, water. At 8 P.M. a glass of water.	Not noted	...	At 11 P.M.	...	..	78·73	1·417
" 30, 31	... ...	...	At 11 P.M., liq. potassæ ʒij, in 3 or 4 ℥ of distilled water.	From 11 to 8 A.M., ℥xivss.	℥j. ʒv.	...	37·087	3·157
August 12	Breakfast at 8—tea, bread, and a small slice of meat.	Tea, ℥xiij.	..	From 8 A.M. to 12 (midday), ℥iij ʒiv.	ʒvij.			
				From 12 to ½ past 1, ℥iss.	℥j.	.	44·39	·906
	Dinner at ½ past 1—meat, bread, potatoes, rice, fruit	℥xij of bitter beer.	...	From ½ past 1 to ½ past 6, ℥iv. ʒiv.	ʒvij		55·59	1·592
	Tea at ¾ to 7—a little bread.	℥xij. of tea.	.	From ½ past 6 to 9, ℥iv	℥j ʒv		56·36	2·219
				From 9 to 11, ℥iss	℥ij. ʒ[illegible].	...	50·17	2·0[illegible]6
" 12, 13	... ...	Water ℥iv. with the liq. pot.	At 11 P.M., liq. potassæ, ʒij.	From 11 P.M. to 2 A.M., ℥xviij.	℥vj.	Not examined for 9 hours, then alkaline from ammonia.	10·18	[illegible]12
" 13	.. ...	.	.	From 2 A.M. to 8 A.M., ℥vj. ʒiv.	℥j ʒv.	Acid	46·49	1·781
	At 8 A.M., breakfast—bread, a small slice of meat.	Tea, ℥xiij.	..	From 8 to 12, midday, ℥iv.	℥j.		31·28	1·238
		Water ℥iii., with liq pot.	At 12 (midday), liq potassæ ʒj.	From 12 to 1, ℥v. ʒiv.	℥v. ʒiv.	Faintly acid.	13·71	·749
	Dinner at ½ past 3—bread, meat, potatoes, fruit.	Bitter beer, ℥x.	..	From 1 to ½ past 3, ℥iiiss.	℥j. ʒiij.	Faintly acid.		
				From ½ past 3 to ½ past 6, ℥iv. ʒvj.	℥j. ʒiv.	Strongly acid.		
		Water, ℥iij. with liq. pot.	At ½ past 6, liq. pot. ʒj.	From ½ past 6 to ½ past 7, ℥j.	℥j.	Acid.		
	Tea at ½ past 7—a very small quantity of bread.	Tea, ℥viij	...	From ½ past 7 to ½ past 11, ℥iv ʒiv.	℥j. ʒ[illegible]	Acid.		2·318
" 13, 14	.. ..	℥iij. water, with liq. pot.	At ½ past 11, liq. potassæ, ʒj.	From ½ past 11 to ½ past 12, ℥viiiss.	℥viijss.	Acid.		·380
" 14	. ..	...		From ½ past 12 to 4 A.M., ℥viij.	℥ij. ʒiv.	Acid.		
				From 4 to 8, ℥iv.	℥j.			
" 15	Breakfast at 8 A.M.	...	...	From 8 A.M. to 10 A.M., ℥ij.	℥j.			
				From 10 to 2 P.M., ℥v.	℥j. ʒij.			
	At 2, dinner—meat, bread, vegetables.	Bitter beer, ℥x.	...	From 2 to 5, ℥ivss.	℥j. ʒiv.			
		At ½ past 5, tea ℥iv. (no solids.)	...	From 5 to 6, ℥iss.	℥j. ʒiv.	...	24·19	1·417
				From 6 to 9, ℥iv.	℥j. ʒijss.			
		At 9, tea ℥iv.	..	From 9 to 11, ℥v.	℥ij. ʒiv			
				From 11 to ½ past 11, ʒvi.	℥j. ʒiv.			

Water, Solids, Sulphuric Acid, and Acidity—**continued.**

Date, 1852.	Meals—time and kind.	Whole amount of liquid taken during the period of experiment.	Liq. potassæ; quantity of, and time when taken.	Time when urine was secreted and passed. Quantity.	Quantity per hour.	Reaction.	Solids per 1000 parts.	Sulphuric acid per 1000 parts.
Aug. 15, 16	.. ..	Water, ℥ij. with liq. potassæ.	At ½ past 11, liq. pot. ʒj.	From ½ past 11 to 2 A.M., ℥xiv.	℥v. ʒiv.	Acid.	5·46	·4511
„ 16	... ..	.	...	From 2 to 3, ℥ij. ʒv.	℥ij. ʒv.			
				From 3 to 8, ℥vss.	℥j. ʒj.	...	27·01	1·407
	At ½ past 8, breakfast — bread and meat.	Tea, ℥xij.	..	From 8 to 10, ℥ij	℥j.			
				From 10 to 12, ℥ij. ʒvj	℥j. ʒiij.			
				From 12 to ½ past 4, ℥v.				
	At 6, dinner—soup, meat, bread, fruit.	Bitter beer, ℥vj.	...	From ½ past 4 to 6, ℥iss.	℥j.			
				From 6 to 8, ℥iv.	℥ij.			
		At 9, tea ℥iv (no solids)	.	From 8 to 11, ℥iv.	℥j. ʒij.			
„ 16, 17	.	Water ℥v., with liq potassæ	At 11, liq. pot. ʒj	From 11 to 1, ℥xj	℥v. ʒiv.	Acid.		
„ 17	... ..	..	.	From 1 to ½ past 2, ℥vss.	℥iij. ʒv.			
				From ½ past 2 to 8, ℥viij.	℥j. ʒiijss.			
	At ½ past 8, breakfast—bread.	Tea, ℥xvj.	..	From 8 to 10, ℥ij.	℥j.			
				From 10 to 2, ℥xij.	℥iij.			
				From 2 to 6, ℥iv.	℥j.			
„ 25, 26	.. ..	...	..	From 12 P.M., to 8 A.M., ℥vj.	ʒvj.			
„ 26	Breakfast at 8—bread, meat.	Tea, ℥xvj		From 8 to 2 P.M., ℥xiij	℥ij. ℈j.			
	Dinner at ½ past 1—bread, meat, vegetables.	Water, ℥x.	..	From 2 to 6, ℥xj.	℥ij. ʒvj	. .	32·02	·7399
	Tea at 6, a very small piece of bread.	Tea, ℥ix.	...	From 6 to 9, ℥ivss.	℥j. ʒiv.			
				From 9 to 10, ℥ij.	℥ij.			
				From 10 to 11, ℥j. ʒv.	℥j. ʒv.	...	48·25	1·578
				From 11 to ½ past 11, ʒv.	℥j. ʒij.			
„ 26, 27 „ 27	.. ...	Water ℥ivss., with the liq. pot.	At ½ past 11, liq. pot. ʒij.	From ½ past 11 to ½ past 2 A.M., ℥xij.	℥iv.	Acid, in 7 hours alkaline from ammonia.	10·16	·5804
				From ½ past 2 to 4, ℥iv.	℥ij. ʒvj.	Acid.		
				From 4 to 5, ʒix.	℥j. ʒj.	Acid.		
				From 5 to 8, ℥ij ʒv.	ʒvij.	Acid.		
				From 8 to ½ past 8, ʒiij.	ʒvj.			
	At ½ past 8—breakfast.	Tea, ℥xviij	...	From ½ past 8 to ½ past 9, ℥iss.	℥ss.			
				From ½ past 9 to 1, ℥vss.	℥j. ʒiv.			
				From 1 to ½ past 3, ℥ij.	ʒvj.			
	At 3, dinner.	Water ℥xiv.	...	From ½ past 3 to ½ past 5, ℥iss.	℥j. ʒij.			
				From ½ past 5 to ½ past 6, ℥x.	℥j. ʒij.			
		At 8 P.M., bitter beer ℥vj.	...	From ½ past 6 to ½ past 11, ℥viss.	℥j. ʒij.			

Water, Solids, Sulphuric Acid, and Acidity—continued.

Date, 1852.	Meals—time and kind.	Whole amount of liquid taken during the period of experiment.	Liq. potassæ; quantity of, and time when taken.	Time when urine was secreted and passed. Quantity.	Quantity per hour.	Reaction.	Solids per 1000 parts.	Sulphuric acid per 1000 parts.
Aug. 28	... ...	...		From ½ past 11 to 8 A.M., ℥viss.	℥j.			
Sept. 1	At 3, dinner - meat, vegetables, bread.	Water, ℥xx	...	From 10 A.M. to 5 P.M., ℥x.	℥j. ʒiij.			
		Water, ℥iv., with liq pot.	At 5, liq. pot. ʒj.	From 5 to 6, ℥j. ʒij.	℥j ʒii.			
				From 6 to 9, ℥v.	℥j ʒv.			
		At 9, tea, ℥x	..	From 9 to 11, ℥vss.	℥ij. ʒij.			
				From 11 to ½ past 11, ʒvij.	℥j. ʒvj.			
„ 2	... ...		..	From ½ past 11 to 8, ℥xx.	℥ij. ʒij.			

On looking over this table, it will be seen that eleven experiments were made—the dose of potash being in one experiment, ʒss., in seven, ʒj., and in three, ʒij. In six of these experiments a large quantity of urine was passed within two hours after the potash was taken; in the other five experiments this effect was not produced. This apparent discrepancy is probably explained by a reference to the time when the potash was taken. When it was introduced into an empty stomach, the large flow of urine followed: when it was taken from three to five hours after food, increase in the quantity of urine only once occurred. In this latter case, in fact, the potash must have been usually neutralized by the free acids present in the stomach, and the resulting chloride of potassium, or lactate of potash, as the case might be, then exerted on the system the action proper to itself, and not that which the liquor potassæ, when absorbed into the circulation before neutralization, invariably produced.

The observations on the 13th and 14th of August may be cited as the best illustration of these facts. On the 13th dinner was taken at half-past three, and the only fluid drunk was ten ounces of bitter beer. For three hours after dinner the hourly flow of urine was ℥iss.; it then declined to ℥j. in every hour. At half-past six, ʒj. of liquor potassæ was taken without effect; eight ounces of tea being drunk at half-past seven, the flow for the next four hours was slightly raised, being ℥j. ʒj. per hour, but it showed no tendency to increase beyond this amount, and was, in fact, beginning to decline, when ʒj. of liquor potassæ in 3 ounces of water was taken. During the next hour no less than ℥viiiss. of urine were passed, a far larger quantity, in the time, than usually follows (in the individual experimented on) the imbibition of a pint of fluid.* During the next three hours the hourly flow declined to ℥ij. ʒiv., which is, however, nearly an ounce above the average. During the next four hours, the hourly flow declined to one ounce, which is considerably below the average.

* In my own case, the quantity of urine passed after an excess of fluid is taken is very much influenced by the condition of the stomach. When the stomach is full, the urinary flow is augmented much less rapidly, and to a much less amount, than when the stomach is empty. When digestion is going on, either the absorption from the stomach is less rapid, or the circulation through the liver is slower, or the water is in firmer chemical combination than when it is taken into the fasting system.

The facts noted on August 15th may be also referred to. In the morning after breakfast the average hourly flow was ℥j. ʒj. At two, dinner and ten ounces of bitter beer were taken. The hourly flow for the next three hours was raised by ʒiij. Four ounces of tea were then taken; the hourly flow declined ʒiss. during the next three hours. Four more ounces of tea being taken, the flow augmented by ʒiss. per hour, continued at this rate for two hours, and then began to fall. One drachm of liquor potassæ and two ounces of water being then taken, no less than 14 ounces were passed in the next two hours and a half, or at the rate of ℥v. ʒiv. per hour.

It is needless to recapitulate the other similar facts of the table. It is sufficient to remark, that if the eye be passed along the columns of tables I. & II., in which the hourly flow of urine is recorded, it will be observed that the highest amount of urine ever passed in one hour did not in nearly 600 hours exceed ℥iij. ʒij. when no potash was taken,* although, sometimes, a considerable quantity of fluid had been previously drunk; whereas in all the six cases in which liquor potassæ was taken on an empty stomach, the hourly flow of urine was almost double this. The following table shows this at once.

III.

Amount of Potash taken.	Quantity of Urine passed immediately afterwards.	Time in which passed.
ʒij.	℥xviij.	3 hours.
ʒj.	℥v. ʒv.	1 hour.
ʒj.	℥viij. ʒiv.	1 hour.
ʒj.	℥xiv.	2½ hours.
ʒj	℥xj.	2 hours.
ʒij.	℥xij.	2½ hours.

Average per hour . . . . = ℥v. ʒvj.
Average per hour when no potash was taken . . . . = ℥j. ʒvj.
Highest single observation when no potash and an ordinary amount of fluid was taken (600 observations) . . . . = ℥iij. ʒij.

The urine thus passed was pale, watery-looking; and in five out of six experiments had a faint acid reaction. In the sixth observation the urine was not tested for nine hours; it was then alkaline from ammonia. In another case, the urine, acid when passed, became alkaline from ammonia in seven hours. This rapid production of ammoniacal alkalinity, if it occur in other cases, has, perhaps, given rise to the erroneous opinion that liquor potassæ produces at once an alkaline urine.

In any given quantity of this urine the amount of solids and of sulphuric acid was, of course, greatly below the average. It becomes a question, however, whether the solids were merely diluted, or were altered otherwise, either in the way of increase or diminution. The following calculation, in which the element of time is equalized, answers this question. The quantities of water, solids, and sulphuric acid, excreted under normal

* Of course it has often exceeded this amount, when large quantities of fluid have been purposely taken.

circumstances in 24 hours, are known.* What then would be the amount excreted, supposing that the flow of urine which occurred after the liquor potassæ, had continued for 24 hours?

IV.

In twenty-four hours there were excreted—

	Water.	Solids.	Sulp. Acid.
	℥. ʒ.	Grs.	Grs.
1. In normal circumstances (average of 16 observations) .	39 vij.	677·84	22·850
2. In 3 hours after ʒij. of liquor potassæ, 18 ounces were excreted; had this continued there would have been in 24 hours	144	703·64	23·598
3. In 1 hour after ʒj. of liquor potassæ, ℥v. ʒv. were excreted, or in 24 hours	132	868·86	45·456
4. In 1 hour after ʒj. of liquor potassæ, ℥viij. ʒiv. were excreted, or in 24 hours	204	...	37·209
5. In 2½ hours after ʒj. of liquor potassæ, ℥xiv. were excreted, or in 24 hours	132	315·94	28·581
6. In 2 hours after ʒj. of liquor potassæ, ℥xj. were excreted, or in 24 hours	132		
7. In 2¼ hours after ʒij. of liquor potassæ, ℥xij. were excreted, or in 24 hours	96	470·20	26·861

Averages of the preceding table, after the exhibition of liquor potassæ.

Water	140, or ℥100 ʒiij. over the normal average.
Solids	597·16, or 80·68 grains below the normal average.
Sulphuric acid . .	32·311, or 9·491 grains over the normal average.

This table sufficiently indicates the increased excretion of sulphuric acid caused by the potash. This may be shown also by comparing the quantities from another point of view. In normal urine, the mean sulphuric acid in 1000 parts was to the whole solids as 1 to 29·66. After the exhibition of liquor potassæ it was as 1 to 18·46. In other words, in 100 parts of solids of normal urine the sulphuric acid constituted 3·37 parts, while in the potash-urine it constituted 5·41 parts.

The only other experiment I have been able to make on a healthy person may be here given. A man, aged 28, dined at 1 o'clock, and took tea at 6. From 9 to 11 he passed ℥iiss. of urine (= ℥j. ʒij. per hour), which contained 3·570 grains of sulphuric acid; this would give, for 24 hours, 42·84 grains. At 11, he took ʒss. of liquor potassæ, and about ℥ij. of water, but no other liquid. From 11 to ¼ to 4, A.M., he passed ℥x. ʒv. of urine (= ℥ij. ʒij. per hour), which contained 9·378 grains of sulphuric acid; this would give, for 24 hours, 47·665 grains. Speaking roughly, about 1 grain more of sulphuric acid was excreted between 11 and 4, than would have been the case had no potash been taken. Thirty drops of liquor potassæ contain about 2 grains of potash, which would have required more than the excess of sulphuric acid to have neutralized it. Either some of the potash was neutralized, or, as far as a single experiment goes, this result accords

* Of course the calculation is not perfectly accurate, as no allowance is made for the increased weight given to an ounce of urine by the solids, the error, which cannot be avoided, is, however, trifling, and is sufficiently equal in all cases.

with an inference drawn from another experiment—that the potash is in union with another acid (perhaps an organic one) as well as with sulphuric acid.

The general characters of the urine after liquor potassæ being, then, that it was highly dilute, with an acid reaction, with an absolute and relative diminution of solids, and with a great relative excess of sulphuric acid, it is time to inquire more fully into its composition. For this purpose two analyses were made.

1. At 2 A.M. on the 15th of August, after ℥j. of liquor potassæ, it will be observed that 14 ounces were passed in two hours and a half. The following was the composition in 1000 parts:

Whole solids			5·460
Organic matter		2·607	
Viz.			
Urea (in undetermined quantity).			
Extractives (in large quantity).			
Uric acid (none).			
Soluble salts:			
(Determined by incineration at as low a temperature as possible)*		2·853	
Sulphuric acid	·4511		
Phosphoric acid	·1012		
Chlorine	1·139		
Potash	·6578		

Heat and nitric acid produced no precipitate, or the very merest haze; ferrocyanuret of potassium had no effect; bichloride of mercury gave a copious precipitate. Trommer's test for sugar gave a negative result.

The organic matter was peculiar. When the residue, after evaporation, was digested in alcohol, a little urea and colouring matter were taken up; a large residue was left. About two-thirds of this residue were soluble in water; the solution had a very strong acid reaction, certainly stronger than that evinced by normal urine treated in the same way; the insoluble portion was insoluble in liquor potassæ, but was nearly entirely soluble in acetic acid.

The organic matter, *after evaporation,* was, then, composed as follows:

1. Alcoholic extract, including urea.
2. Water extract.
3. Substance insoluble in water and alcohol, but soluble in acetic acid.

It is not easy to assign the exact composition of the soluble salts. The sulphuric and phosphoric acids may be supposed to have been united with potash, and would therefore absorb of it ·5309 and ·0668 respectively. This would leave ·0601 of potash, an amount quite insufficient for the quantity of chlorine, which would, indeed, have required more than double the whole amount of potash. It may be assumed, therefore, that the chlorine was united, as usual, with sodium. This would give the following hypothetical arrangement:

* The soluble salts were dissolved out, evaporated and weighed, the sulphuric and phosphoric acids were determined both before and after incineration. The earthy salts were in too small a quantity to be weighed.

Chloride of sodium	. . .	1·8875
Sulphate of potash	. . .	·9820
Phosphate of potash	. . . .	·1680
Excess of potash	. . . .	·0601

Either the excess of potash existed as potassium with chlorine, or it was combined with an organic acid destroyed by heat. This last supposition is strengthened by the fact, that the watery solution of the organic matter in this and in the succeeding analysis, left undissolved by the alcohol, was strongly acid.

A careful examination was made to see whether any sulphur existed, except in the state of sulphuric acid, and this was found not to be the case. With reference especially to the amount of potash, the whole 14 ounces must have contained rather more than four grains, which is almost precisely the amount contained in 60 drops of liquor potassæ.

2. An examination was made of the urine passed at half-past 2 A.M., August 27th.

On the 26th, dinner was taken at half-past 1, and tea at 6. The urine passed from 10 to 11 P.M. had the following composition in 1000 parts:

Solids	. . . .	48·25
Chlorine	. . . .	5·180
Sulphuric acid	. . . . .	1·578

At half-past 11, liquor potassæ ʒij. and water ℥ivss. were taken. In three hours ℥xij. were passed, of the following composition:

Solids			10 16
Viz.			
Organic matter		5·371	
Salts by incineration		4·789	
Soluble salts·			
Sulphuric acid	·5804		
Phosphoric acid	·2756		
Chlorine	1·554		
Potash	1·573		

In the twelve ounces there would be contained about 8·1 of potash. Two drachms of liquor potassæ contain a little more than eight grains, so that all the potash had passed off. If the same rules are followed as in the former analysis, the hypothetical arrangement is as follows:

Chloride of sodium	. . .	2·572
Sulphate of potash	. . .	1·2636
Phosphate of potash	. . .	·4755
Potash (in surplus)	. . .	·690

The examination of the organic matter gave the same results as in the former case: there was no uric acid; urea was in small amount; there was a large amount of extractives, soluble after evaporation in alcohol, or in water, or in acetic acid.

If these two analyses are compared, the results are found to be very similar, except that the quantity of water was much greater in the first than in the second observation. In both cases, the ratio of fixed saline matters to the organic matter was extremely large, and sulphate of potash and chloride of sodium constituted the chief proportion. In both, the phos-

phoric acid was in moderate amount. In both, the organic matter was peculiar.

In these two analyses the proportion of chlorine to the other solids is relatively large, but it does not appear that there is an absolute increase in the excretion of the chlorine, as there is of the sulphuric acid. Thus, using round numbers, in the hour from 10 to 11 P.M. on the 25–26th, 4 grains of chlorine were passed, or at the rate of about 96 grains in the 24 hours; in the three following hours after the liquor potassæ, about 6·5 grains of chlorine were passed, or at the rate of only about 51·5 grains in 24 hours.

The mode in which liquor potassæ produces these results is easily understood. It was first observed by Chevreul, and afterwards by Scherer, that many organic substances, and among others albumen and hæmatin, when dissolved in water, are little acted upon by the oxygen of the air under ordinary conditions, but are oxidized with extraordinary rapidity when a little alkali is added.* It has become generally admitted that the same result occurs in the animal organization; and Lehmann, in his masterly work, has lately handled this subject with his accustomed power. Without entering here into this question, it is sufficient to remark, that my experiments are entirely confirmatory of this opinion, and the evident oxidation of sulphur proves satisfactorily, that in a healthy condition of the system the albuminous substances are those which are acted upon when an excess of alkali is present. The sulphur passes into the state of sulphuric acid, and is excreted by the kidneys. Although such a fact is by no means proved, it may be that the albumen or fibrine, thus deprived of an essential element, appears in the urine as the peculiar extractives formerly mentioned.†

The immense flow of urine which follows the full action of potash cannot be attributed to the sulphate of potash which is formed. It is much more likely to be owing to the altered albuminoid substance.

What protein compound is thus oxidized, albumen, fibrine, or globulin, is not certain, and it is not impossible that one or other substance may be attacked, according to circumstances.

Such is the action of liquor potassæ on the system, when it is absorbed unneutralized into the blood. Such probably is the action likewise of carbonate of potash, as it must be assumed that the liquor potassæ is at once converted into carbonate when it enters the circulation. Some of the other salts of potash do not, however, have this effect. A series of careful experiments on my own person with large doses of nitrate, and of acetate, of potash, have shown that these salts cause no increase of the sulphuric acid, nor of the solids generally.‡ The effect of iodide of potassium has not been determined.

I must now refer to some negative experiments. It has already been

* The latest notice I have been able to find of the presumed action of alkalies is by Ruete (Lehrbuch der allgemeinen Therapie. Gott. 1852), in which it is stated that unneutralized alkalies probably enter the blood in combination with the albumen of the secretion of the stomach and intestines, and with fibrine. These combinations are said to soon decompose, and the alkali emerges from the body as carbonate, lactate and chloride. The fibrine in the blood diminishes. Nothing is said of sulphuric acid, and I do not know on what experiments Ruete bases his conclusions.

† Very lately Verdeil (Chimie Anat. vol. iii. p. 299) has shown that when albumen is boiled for three days with a very small proportion of potash or soda, an azotized acid is formed.

‡ This result, in the case of the acetate of potash, is the more remarkable, as this salt causes the appearance of a large quantity of alkaline carbonates in the urine, and renders the urine as certainly alkaline as the tartrate or the citrate.

shown by five experiments, that when liquor potassæ is taken during the process of digestion, its peculiar effects are not produced.

1. In the first (March 26), dinner was taken at 6, and ʒss. of liquor potassæ was taken at 11, i. e., between four and five hours after the conclusion of dinner. No effect whatever was produced on the water, solids, or sulphuric acid.

2. In the second experiment (March 28), dinner was taken at 7, and ʒj. of liquor potassæ at 11. The urine passed at 11 contained 1·618 of sulphuric acid; that passed at 3 A.M. contained 2·327. This increase in the sulphuric acid cannot be attributed with certainty to the potash, as the influence of food was not sufficiently excluded. No effect was produced on the water.

3. In the third experiment (March 30), dinner was taken at half-past 5, at 8 a glass of water was taken, at 11 ʒij. of liquor potassæ. The urine at 11 (urina cibi) contained 78·73 of solid, and 1·417 of sulphuric acid in 1000 parts, that passed at 8 A.M. contained only 37·087 of solid, and no less than 3·157 of sulphuric acid. In this case there can be no doubt that the sulphuric acid was augmented, and it is possible that the water may also have been increased, as it was not passed for nine hours, and the increased flow of the first two hours may have been neutralized by the diminished amount afterwards passed. This observation, therefore, although it cannot be used, corroborates the results previously arrived at.

4. In the fourth experiment (August 13), dinner was taken at half-past 3, and ʒj. of liquor potassæ at half-past 6. No effect was produced on the water.

5. In the fifth experiment (September 1), dinner was taken at 3, and ʒj. of liquor potassæ at 5. No effect was produced on the water.

The following table carries these experiments a step further. In it are recorded the effects of liquor potassæ when given in the ordinary routine way, three times daily, on the water, solids, and sulphuric acid:

V.

Effects of Liquor Potassæ, given without regard to Food.

Date, 1852.	Liq. potassæ: quantity and time.	Time when urine secreted and passed.	Quantity per hour in round numbers.	Solids per 1000.	Sulphuric acid per 1000.	Total solids in 24 hours.	Sulphuric acid in 24 hours.
March 31	During the day 120 minims in 3 doses.	From 10 P.M. to 9 A.M. (1st), ℥xj.	℥j.	54·54	2·9		
Mar 31, April 1.	... ...	From 9 A.M. to 9 A.M., ℥xxxij.	℥j. ʒij.				
April 1	During the day 120 minims in 3 doses.	From 11 P.M. to 7 A.M., ℥viij.	℥j.	61·38	2·981		
„ 1, 2		From 9 A.M. to 9 A.M., ℥xxxij.	℥j. ʒiss.				
„ 2	During the day 120 minims in 3 doses.	From 12 P.M. to 8 A.M., ℥vij.	ʒvij.	57·53	2·686		
„ 2, 3		From 9 A.M. to 9 A.M., ℥xxxiv.	℥j. ʒiiiss.				
„ 3, 4	None	From 11 P.M. to 9 A.M., ℥viij.	ʒviss.				
		From 9 A.M. to 9 A.M., ℥xlj.	℥j. ʒvss.				

Effects of Liquor Potassæ, given without regard to Food—continued.

Date, 1852.	Liq. Potassæ: quantity and time.	Time when urine secreted and passed.	Quantity per hour in round numbers.	Solids per 1000.	Sulphuric acid per 1000.	Total Solids in 24 hours.	Sulphuric acid in 24 hours.
June 6	At 6 and 11 P.M., 20 minims.						
„ 6, 7	... ...	From 11 P.M. to 8 A.M., ℥xss.	℥j. ʒj.	30·25	1·514		
„ 7	During the day 40 minims.						
„ 7, 8	... ...	From 12 P.M. to 8 A.M., ℥viiiss.	℥j. ʒss.	53·31	2·321		
		From 8 A.M. to 8 A.M., ℥xliij.	℥j. ʒvj.	40·53	1·189	836·53	24·540
„ 8, 9	None....................	From 12 P.M. to 8 A.M., ℥xss.	℥j. ʒiiss.	43·18	1·883		
		From 8 A.M. to 8 A.M., ℥xxxiij.	℥j. ʒiij.	53 04	1·635	840·15	25·898
July 26	During the day 40 minims.						
„ 26, 27	... ...	From 11 P.M. to 8 A.M., ℥xiiss.	℥j. ʒij.	38·16	1·513		
		From 8 A.M. to 8 A.M., ℥xxxiij.	℥j. ʒiij.	41·99	1·295	665·12	20·512
„ 27	During the day ʒj. in 3 doses.						
„ 27, 28	... ...	From 11 P.M. to 8 A.M., ℥xviij.	℥ij.	22·37	·954		
		From 8 A.M. to 8 A.M., ℥xliij.	℥j. ʒvj.	21·07	·935	434·88	19·298
„ 28	During the day ʒj. in 3 doses.						
„ 28, 29	... ...	From 11 P.M. to 8 A.M., ℥xlij.	℥j. ʒiiiss.	35·77	1·501		
		From 8 A.M. to 8 A.M., ℥xxxvj.	℥j. ʒiv.	34 26	1·118	592·01	19·319
„ 29	During the day ʒj. in 3 doses.						
„ 29, 30	... ...	From 11 P.M. to 8 A.M.. ℥xvj.	℥j. ʒvj.	23·09	1·047		
		From 8 A.M. to 8 A.M., ℥xlj.	℥j. ʒviss.	27·03	1·022	531·95	20·112
„ 30, 31	None....................	From 12 P.M. to 8 A.M , ℥xx.	℥ij. ʒij.		·800		
		From 8 A.M. to 8 A.M., ℥xliv	℥j. ʒvj.		·980		20·697

Averages.

	Morning Urine.	
	Urine of Table V.	Normal Morning Urine—Table I.
Quantity per hour	℥j. ʒij. ℈j.	℥j. ʒvj. ℈ij.
Solids per 1000 parts ...	41·95 (10 obs.)	37·27
Sulphuric acid per 1000...	1·827 (11 obs.)	1·4608

Averages.

	Urine of 24 Hours.	
	Urine of Table V.	Normal Urine of Table I. (16 obs.)
Quantity	℥xxxviij. ʒiv. (11 obs.)	℥xxxix. ʒvij.
Quantity per hour	℥j. ʒivss.	℥j. ʒv. ℈j,
Solids per 1000	36·32 (6 obs.)	35·41
Sulphuric acid per 1000...	1·167 (7 obs.)	1·190
Solids in 24 hours	650·10	677·84
Sulphuric acid in 24 hours	21·482	22·850

The close correspondence of the two series last given, proves that the average amount of the water, solids, and sulphuric acid, already assigned to normal urine (in the individual experimented on), must be very near the truth. It also proves, that these quantities were not affected by the exhibition of liquor potassæ, given indiscriminately in small doses during the day.

The effect produced by liquor potassæ on the healthy system may be thus recapitulated. If this remedy be taken soon after meals, its action is that of an antacid. It combines with hydrochloric or with lactic acid, and then, doubtless, passes into the circulation. What appreciable effect it now produces is not indicated in the tables above given, but it does not increase either the water, solids, or sulphuric acid of the urine. If the liquor potassæ be taken into an empty stomach, it passes unneutralized into the circulation, and probably through the veins; in so doing it must produce an effect on the walls of the capillaries and small veins, but the extent of this cannot be known. As much as ʒij. have been taken with only 4oz. of water, without causing epigastric pain or uneasiness (although it produced considerable temporary scalding of the mouth and throat), and without apparently producing any local effects in the stomach. In, usually, from thirty to ninety minutes after its entrance into the circulation,* an increased flow of slightly acid urine occurs, which contains the whole of the potash, organic matter differing considerably from that of ordinary urine, and a relatively large proportion of sulphuric acid; the phosphoric acid and the chlorine are less changed. Perhaps an organic acid (not uric, and probably not hippuric) is also present. The explanation of these facts is, that an albuminous compound, either in the blood itself, or in the textures, has become oxidized; its sulphur, under the form of sulphuric acid, has united with potash, and, with possibly the changed protein-compound, is poured out from the kidneys. This oxidizing effect of the liquor potassæ is no doubt assisted by exercise, and by copious draughts of water; but in the above experiments, exercise and fluid were abstained from, in order not to complicate the results. The amount of albumen or fibrine destroyed by

* This does not sufficiently appear from the facts in the text. I subjoin some particulars of another experiment, made when the system was not quite in a state of health. At a quarter past 4, liq. potassæ ʒj, water ℥iv, was taken.

At a quarter to 5, ℥ij. ʒvj. were passed . . . = ℥v. ʒij. per hour.
At a quarter past 5, ℥iv. ʒvij. were passed . . = ℥ix. ʒvj. per hour.
At a quarter to 6, ℥j. ʒij. were passed . . = ℥ij. ʒiv. per hour.

one drachm of liquor potassæ cannot be considerable, but if the potash were continued in large quantities, oxidation could probably be pushed to any amount. The nitrate and acetate of potash did not in a *healthy system* have the same effects.

After the increased flow of urine, the quantity passed per hour falls slightly below the standard. It appears to resume its ordinary composition, but its exact condition at this period has not been determined. Some observations on urine in disease, would lead me to infer that the uric acid will be found to be increased.

Such were the effects of liquor potassæ on the urine. The effect produced on other excretions was not obvious. The skin and the intestines appeared quite unaffected, and as all the potash was found in the urine, the reason of this is easily understood. In most of the experiments there were no subjective symptoms of any kind. On two occasions, there was rather sharp frontal headache, languor, depression, slight lumbar pain, and aching of the legs, after the large flow of urine. On the night of the 15th, when the flow of urine, which was proceeding at the rate of ℥iss. per hour, was augmented in two and a half hours by ℥xiv., and no fluid was supplied to the system, the pulse became perceptibly small (almost thready) and slow; it remained equal and regular—there was no thirst, no shivering, and no nausea; the skin was dry and warm. In six hours the pulse had quite regained its force and frequency, and the other symptoms had disappeared without any fluid having been taken.

After the experiments were concluded, the general health did not appear impaired; it was, if anything, better than usual.

The effect of liquor potassæ on the diseased system is a much more difficult problem. The chemical conditions are not the same, and the effects of the potash are necessarily influenced by them. I will not now enter into this subject, but observe that it is necessary, when its oxidizing effects are desired, to give the potash eight or ten hours after food, to drink moderate quantities of water, and if possible, to use exercise. The potash should be given pure, or with large doses of iodide of potassium, but unmixed with sugar. I may so far anticipate what will be hereafter said on this point, by stating that, administered in this way, it exerts a powerful effect on the exudations of inflammations, but appears less useful in the early stages, when an antagonistic force seems to be in action.

It remains to be seen whether the varying excretion of sulphuric acid, which is unaccounted for by diet and exercise, is occasioned by greater or less alkalinity of the blood producing variations in the amount of oxidation of the albuminous compounds.

PART FOURTH.

Chronicle of Medical Science.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Increased Frequency of the Contractions of the Heart by Direct Irritation. By Dr. T. BUDGE.

IN a preliminary communication Dr. Budge states that the frequency of the contractions of the heart is constantly increased by stimulating directly the sympathetic nerve in its course below the heart Previously to beginning the experiment, it is necessary to destroy the connexion between the medulla oblongata and the heart, either by decapitating the frog or by dividing the nervus vagus of each side. Dr. B. waited always for the whole of an hour after the decapitation, or six or eight hours after the section of the nervi vagi, ere he commenced to apply the stimulus, for which he used the apparatus of *Dubois Reymond* (which permits the experimenter gradually to augment or to diminish the degree of irritation according to convenience). The increase amounted from 8 to 12 and 24 contractions in a minute. The same effect was produced by irritating the posterior part of the lower end of the spinal marrow. Dr. B. performed this experiment by applying the poles to the denuded bone at the junction between the os coccygis and the last vertebra, without laying open the spinal marrow. He further repeatedly observed, that even in eight or nine hours after the destruction of the spinal marrow, neither by direct nor by indirect irritation could any contraction be effected in the heart, when by the same degree of irritation complete tetanus was still produced in the muscles of the extremities.—*Froriep's Tagesber.*, No. 441, 1852.

On Stagnation of Blood in the Web of the Frog's Foot. By Dr. H. WEBER.

IN order to test the accuracy of the opinions which ascribe a great importance to the action of the heart and the condition of the vessels in stagnation of blood produced artificially in the vessels of the web of the frog's foot, the author studies these phenomena both when the circulation is free and when it is mechanically arrested, by placing a ligature round the leg or thigh. He brings a certain portion of the web in the field, cuts the ischiatic nerve, or narcotizes the frog, or takes out the spinal cord in order to prevent movements, and then applies a ligature. When the circulation is fully arrested, which does not occur for some time, in consequence of the contractions and dilatations of the artery giving rise to oscillatory movements, he applies to the web various irritating chemical agents. He finds that these agents produce exactly the same effects when the circulation is arrested as when it is free, and when the nerve is cut as when it is entire. The phenomena noticed during the free circulation are as follow:—Dilute potash or ammonia applied to the web causes extreme contractions of the arteries; the diameters of the veins remain unchanged; soon the blood in the veins begins to move more slowly, then stops, then oscillates, then finally commences to flow backwards towards the capillaries. This backward flow in the veins lasts until all the capillaries touched with the solution are in a state of complete stagnation. That this reverse current is not owing to diminished pressure on the arterial side by reason of the contracted

artery, is proved by the fact that solution of salt causes the same reverse current, although the arteries are dilated by its use and not contracted. That the reverse current in the veins is not owing to altered pressure from the heart or from arterial changes, is proved by this fact, and also by the occurrence of the phenomena when the circulation is arrested by ligature.

When the ligature is applied, the circulation in the part below is of course arrested, and after a time the blood is quite tranquil: after a still longer interval, however, (four to eight hours) the blood commences to move again, and passes directly from the arteries, and reversely from the veins into the capillaries; in this way, without the application of irritating agents, stasis occurs. This stasis is, however, incomplete, since on removing the ligature the heart's action has sufficient power to force on the stagnant blood, and thus to free the circulation. Very different is the case when reagents are employed. If to the vessels in the web of the ligatured foot, as soon as the movements of the blood have stopped, a solution of potash or ammonia, or hot water, or dilute acetic acid, or rock salt, urea, nitre, carbonate of soda, or chloride of calcium in cold saturated solution, is applied, at once the blood in both arteries and veins commences to flow towards the capillaries, in which vessels the blood-corpuscles are pressed closer together, as fresh ones are added to them: not only the capillaries but the little arteries and veins are thus filled. When this stasis is fully completed, if the ligature is removed the heart's action cannot at once force on the blood and clear the way—the blood-particles must first undergo the well-known changes; they become paler, disclose their nuclei, and finally begin to get loose from each other, to oscillate, and at last to be carried away in the torrent of the circulation. Other reagents, as syrup, or *very dilute* sulphuric, nitric, hydrochloric and nitric acids, cause a stasis similar in all respects, except that when the ligature is removed the blood-stream is sufficient at once to carry away the stagnant blood. If these reagents are tried at once on an unligatured web, they do not cause any stasis; on the contrary, they cause the superficial capillaries to become empty of blood, while in the deeper-seated a rapid circulation continues. Some other reagents, such as phosphate of soda and borax, have no effect at all on the blood-movements. The author concludes, that the heart's action and the continuance of the circulation of the blood are not necessary conditions to the stagnation, but that this occurs in consequence of certain movements in the blood itself.—*Muller's Archiv.*, 1852, Heft 3, p. 361.

On Blood-Corpuscle-Holding Cells. By M. VIRCHOW.

[MR. WHARTON JONES'S Review (p. 32) is purposely restricted to the consideration of the blood-corpuscle-holding cells of the spleen, and to the physiological doctrines which have been based upon the observations of Kölliker and Gerlach, and their respective followers. Virchow treated the question on the broader basis, and discusses the origin of these cells without especial reference to the seat in which they were first noticed.]

The author commences his paper with a discussion on the present opinions of cell-growth, and on the application of these opinions. 1. The blood-corpuscle-holding cells. Against the existence of these cells he had formerly argued that it was impossible to formularise, according to the known laws of cell-formation, the presumed envelopment of a heap of blood-cells by a cell-wall, and the subsequent conversion of a cell so formed, into an actual nucleated cell. He did not, however, deny the possibility of cells being thus formed, nor did he affirm the universal truth of the cell-formation described by Schwann, nor did he reject the possible origin of cells by cleavage of nuclei. Still, admitting these modes of formation, it is possible to frame a formula which shall include all known facts—viz., for cell-formation it is necessary to have a *cyto-blastema* of determined chemical composition, and *centra* round which the formation occurs. As blastema we only recognise the so-called fatty-albuminous histogenetic matters, which are

all amorphous: there are no facts to show that organized tissues, or entire cells, as bloodvessels or cerebral substance, can serve as cyto-blastema. In addition to the blastema, there is in all cases an external influence necessary (such as the contact and action of living tissues, &c.) which calls forth in the blastema those combinations which develop in it the power of organization.

2. After discussing these points at great length, and showing that the formation of the blood-corpuscle-holding cells cannot be brought under the same formula, the author passes on to a consideration of pigment-building. Pigment-grains were formerly supposed by Virchow to arise in two ways, either in the interior of the single or aggregated shrunken blood-particles, or from the passage of hæmatin out of blood-particles into other textures. But in addition, the author's later researches have proved that pigment-building may occur in fat, by imbibition of biliary colouring matter, &c. It is evident, therefore, how cautiously any inductions must be drawn from the existence of pigment-grains in cells; and if such pigment-grains resemble shrunken blood-particles, this may yet be a mere deception, and may not prove that the particles are first enclosed in cells, and then pass into pigment.

3. After these preliminary considerations, the author arrives at the practical examination of the subject. He formerly denied the existence of blood-corpuscle-holding cells, as he had never been able to find them. *He now, however, admits their existence: he has found, though infrequently, cells with nuclei and decided blood-corpuscles in the spleen, has isolated them, and rolled them over in the field of view.* In tumours they are much more common.

Admitting, then, the existence of these cells, how are they formed? The opinions of Kölliker have been shown to be doubtful. The bold conjecture of Rokitansky, *that these blood-corpuscles are new formations in the cells*, is not adopted, though the author does not reject it, and even seems inclined to admit its probability. As some evidence in its favour, he describes a remarkable cell discovered in a fatty liver; the greater part of the cell was occupied by a clear round body resembling the cavity described by himself in some cancer-cells. Within this body or space were more than a dozen corpuscles disposed round a hyaline bullet-shaped body; between the cavity and the outer cell-wall were two nucleoli-like bodies; the rest of the cell was in a state of fatty degeneration. Were these corpuscles new formations, or were they enclosed by this double wall laid around them?

But besides these two hypotheses, a third may also be proposed. May not the corpuscle-holding cells arise by the entrance from without of blood-corpuscles into cells already formed? As in the experiments of Œsterlen, Mensonides, and Donders, solid particles pierced the walls of vessels, and penetrated by pressure through tissues; so may not also the heavy and tough blood-corpuscles break through the tender walls of new-formed cells, and thus gain access to their interior? The hypothesis is backed by no positive facts, but as some evidence that such a thing is possible, Virchow refers to an observation of E. H. Weber, who having injected the liver, found some liver-cells filled with injection: in some of the cells the place of entrance of the injection could be seen, in others no opening could be found.—*Virchow's Archiv.*, B. iv. Heft 1.

The Nerves of the Heart. By Cloetta.

The author has examined the nerves in the hearts of men, calves, and oxen, in order to test the accuracy of the description of Dr. Robert Lee, which he in great part confirms. The nerves passing down from the great plexus between the aorta and pulmonary artery distribute themselves partly to the tissue, and partly form numerous ganglia in the auriculo-ventricular groove, as described by Lee, and as previously noted by Remak. These superficial nerves are very soft and tender, and the author has not found them so numerous as Lee did. The author doubts whether the term "fascia cordis" should be applied to the thick uniting tissue; and he states also, that the swellings formed by the nerves crossing the vessels are not ganglia, although they have the greatest external resemblance to them. They

contain, however, no ganglion-cells. The author confirms Lee's statement, that the left ventricle is more richly supplied with nerves than the right, as is best seen in oxen. Whether in hypertrophy the nerves grow, he has not determined.—*Wurzburg Gesell. Verhandl.*, B. iii. Heft 1, p. 64.

On Vierordt's method of Blood-analysis. By SCHMIDT.

[THE method of determining the number of red-corpuscles proposed by Schmidt,* has been attacked by Vierordt, who has himself brought forward a new method, which is now criticised by Schmidt. Vierordt proposed to *count*, under the microscope, the number of blood-globules, as seen in a certain capillary tube of known dimensions. We need not enter into the details of this mode, nor into the strictures made upon it, but merely indicate the present paper to those who are interested in this important subject.]—*Henle's Zeitschrift*, Band ii. Heft 3, p. 293.

Crystals in Blood. By KUNDE and FUNKE.

[These are two elaborate papers on the crystals which may be obtained from blood under the microscope by the addition of a small quantity of water, alcohol, ether, &c., to various kinds of blood, both from men and from the lower animals. We defer their analysis, as we intend to review, shortly, all the observations which have been made on crystals in organic fluids. We may notice, only, that although the inquiry is as yet merely in its infancy, it is sufficiently advanced to give us some hope that it will not be barren of results, but will eventually throw some light on the nature of the fluid of the red corpuscles. Both the authors notice the extreme difficulty of obtaining the crystals in quantity, and this is at present a great bar to a satisfactory chemical investigation.]—*Henle's Zeitschrift*, Band ii. Heft 3, pp. 271 and 288.

Crystals of Hæmatoidin in the Bloody Fluid of a Tumour. By DR. BACON.

IN the bloody fluid obtained by puncturing a large cancerous tumour with an exploring needle, the author observed cancerous elements, blood-discs, and rhombic crystals, of a fine transparent crimson and ruby-red colour. No chemical reactions were observed. In a few hours the crystals had entirely disappeared. The author considered them the hæmatoidin-crystals of Virchow.—*American Journal of Med. Science*, Oct. 1852.

[Some little confusion seems likely to arise, unless care be taken, about these crystals. The hæmatoid-crystals of Virchow are broadly distinguished by their extreme stability and their comparative indifference to reagents. The crystals described above are evidently similar to those noted by Funke in the splenic blood of the horse, and afterwards, and almost simultaneously, by Kunde, Funke, and Parkes, in human blood. These crystals are of another order, and are distinguished by their extreme destructibility.]

The Laws regulating the Bodily Temperature and the Frequency of the Pulse. By R. LICHTENFELS and R. FROHLICH.

THE authors have made a most careful series of experiments on themselves. Each experimenter is twenty-two years of age; the pulse of one of them is normally 71 per minute, that of the other 88; the normal temperature of each is 98·134. During the course of the experiments, they rose shortly before 7 A.M., took coffee between 7 and 8, had dinner at 2, and evening-coffee between 7 and 8.

1. *Daily rate of pulse, and temperature.*—The influence of the period of the

* Charakteristik der epid. Cholera. Leipzig, 1850.

day, *per se*, was very trifling, but both pulse and temperature were greatly affected by food. Before the morning-coffee the pulse was lowest; by the end of the first hour after coffee it rose, on an average of many observations, nearly 8 beats per minute; it was slightly less rapid at the end of the next hour; at the end of the third hour it was only 3·3 beats; and at the end of the fourth, 2·77 beats over the original number. The pulse did not sink to the number noted before coffee, till six hours had elapsed. The mid-day meal raised the pulse again, and this occurred apparently sooner after protein than after starchy food, but to a less extent. After the evening-coffee, the pulse, which had fallen, again rose, but to a less extent, and its declension occurred more rapidly.

The temperature of the body was affected in a similar way by food, but the augmentation occurred later than the rising of the pulse; so that the temperature was often at its maximum when the pulse had fallen considerably towards the point from which it had risen. The average amount of increase is about $\frac{1}{3}$ of Fah. The greatest average range of the thermometer in the course of the day (between 7 A.M. and 10 P.M.) was rather less than a degree of Fah.

2. *Influence of customary liquid.*—The experiments were performed in the afternoon; each lasted 100 minutes, and the greatest tranquillity of body was preserved. After *beer*, the pulse sank 6 or 7 beats in from 10 to 15 minutes; in 30 minutes, it regained its former frequency; much before this time, the subjective feelings of slight incipient intoxication were felt. In about 2 hours, the pulse was heightened nearly double as much as it had been depressed. The temperature, after the use of beer, fell about one-third of a degree of Fah. After *wine*, the pulse at first fell in the same way, and then rose greatly; the temperature fell about half a degree of Fah. The same occurred with alcohol, but afterwards the temperature rose about a quarter or half of a degree of Fah. Cold water lessened, at first, the number of the pulse, and lowered the temperature. In 15 minutes both returned to their former amount. Coffee, as already said, raised the pulse, but more in the morning than in the evening.

3. *Influence of fasting.*—Fasting for from 20 to 21 hours lowered both pulse and temperature. At the end, the pulse was from 12 to 16 per minute; the temperature as much as 1·8° Fah., under the normal. The curious observation (made also by Davy and Gierse) was noted, that at the period of customary meal-times both pulse and temperature slightly rose.

4. *Influence of muscular movements.*—Various experiments were tried with different kinds of movements. 1. A ten-pound weight was allowed to hang from the arm for five minutes, the body being tranquil; the pulse first fell in frequency, then rose; its greatest frequency was after the termination of the experiment. When the weight was on the left arm, the rise was nearly double that which occurred when it was on the right arm. 2. A weight of one pound was held out horizontally; the pulse rose and fell remarkably several times. 3. A weight of two pounds was rapidly swung round and round with one arm, while the other was placed on a table, that the pulse might be counted. This exercise produced the greatest effect on the pulse, raising it sometimes from 30 to 50 beats. 4. Long-continued moderate exercise, carried on to fatigue, raised the pulse greatly for some considerable time, but never produced the enormous rise noted in the previous kind (3) of muscular exertion.

5. *Influence of narcotic poisons.*—*Belladonna* and *atropine* at first diminished the frequency of the pulse (16 to 20 beats), but after a variable time (50 to 117 minutes), the pulse again rose (12 to 30 beats). The smaller doses produced greater primary sinking than the large, but required much longer time to do so; on the contrary, the larger doses produced much greater secondary rising; that is to say, the maximum sinking-point is inversely, and the maximum rising-point is directly, proportioned to the amount of the drug. It might be said that small doses depress, larger excite, the pulse. The temperature was diminished in all cases. *Opium*, especially in small doses, caused rising of the pulse, but afterwards there was great sinking, and the temperature diminished. The *Cannabis Indica* produced many

periods of rising and falling; the temperature rose for about four hours, and to as great a degree as ·7° or 1° Fah. *Chloroform* and *ether*, if not pushed to too deep narcotism, raised both temperature and pulse.—*Denkschrift. d. math-naturw. Klasse d. k. k. Akad d. Wiss zu Wien; und Schmidt's Jahrbuch*, 1852, Oct. No. 2.

Contractile Tissue of the Iris. By JOSEPH LISTER, Esq.

IN a fresh portion of human iris removed by operation, the author was able (in certain places) to isolate muscular fibre-cells, of which he gives representations. The length of the longest cell was 1-125th of an inch, and its breadth 1-3750th; others were shorter, but nearly as broad. He confirms Kölliker's statement of the existence of a sphincter pupillæ in the eyes of men, rabbits, guinea-pigs, and horses; and describes it as consisting, at the pupillary margin, of contractile fibre-cells without any uniting tissue. The author is inclined to doubt whether the individual cells are united end to end into fibres, as they are separated with great ease. As regards the dilating fibres, the author thinks that the fibres described by Bowman as contractile are the outer cellular coats of the vessels; but he has been able to discover in the outer part of the human iris, "long delicate fasciculi, whose faint outline, absence of fibrous character, and possession of well-marked elongated nuclei parallel to the direction of the fibre," convinced him they were muscular fibre-cells. In the horse, also, these cells can be seen, when from the anterior surface of the iris a tough membrane ("composed of short felt-like fibres gelatinized by acetic acid") is peeled off. The muscular fibre-cells of the dilator can be seen, running from the outer part of the iris, at various angles with the sphincter, and finally making a short curve, and blending with it. The fibre-cells of the dilator are held together more firmly than those of the sphincter, and therefore cannot be defined satisfactorily.—*Microscopical Journal*, No. 1, p. 8.

Contribution to the Knowledge of the Elastic Tissue. By DR. ZOLLIKOFER.

ZOLLIKOFER studied principally the products of decomposition of this tissue. After having perfectly cleared the ligament-nuchæ of the ox of fat and intercellular tissue, he boiled it from 48 to 50 hours with diluted sulphuric-acid, saturated the clear brown solution with aqua calcis, and then boiled the whole mass, which had become white and pappy; during the process of boiling it lost its colour, and developed a smell similar to that of the flowers of berberis. After the separation by filtration of the gypsum, and the inspissation of the fluid to the consistency of syrup, besides the salts of lime, a yellowish, granular, crystalline sediment formed, which, when washed out, and repeatedly boiled with alcohol for the separation of the salts of lime and of the colouring matter, was recognised as *leucin*; it presented the following characters: crystallized out of alcohol (93°), it formed a perfectly white crystalline powder, glittering like mother-of-pearl, of a greasy touch, without smell and taste, and grating between the teeth. According to the strength and concentration of the alcohol, from which it deposited, the crystals had sometimes more the form of pillars, at other times more that of rhomboidal tables. As characteristic for the leucin obtained in this way, Zollikofer considers the formation of concentric, rosette-like groups of crystals, in which each single crystal is to be more or less distinctly recognised. By carefully heating to about 400° Fah., it may be completely sublimated, without melting, in the form of a white mist, smelling like burnt horn. It dissolves in about 27 parts of cold, or a rather smaller quantity of hot water, in 1040 parts of cold alcohol of 96°, and in 800 parts of hot alcohol of 98°; it is insoluble in ether, but most easily soluble in ammonia. The solutions of leucin do not act on vegetable colours, nor are they precipitated by any reagents; with sulphate of copper and liq. potassæ it produces a light blue solution, which undergoes hardly any change by boiling; in

concentrated nitric acid it dissolves without disengaging any gas, and an acid combination of leucin and nitric acid (leucin-salpetersäure) precipitates from this solution in crystalline crusts; rhomboidal tables, of a silk-like gloss, are obtained from the solution in hydrochloric acid. The elementary analysis of leucin shows the following composition:—C 54·96, H 9·93, N 10·68, O 24·42 = $C_{12}\ H_{13}\ N_1\ O_4$.—*Ann. der Chem. und Pharm.*, vol. lxxxii. Heft 2, 1852.

On the Influence of the Sympathetic Nerve on the Animal Temperature. By Dr. T. Budge.

To the communication of Bernard's observation, that by dividing the sympathetic nerve between the first and second ganglion cervicale, the temperature of the corresponding side of the head soon rises several degrees, and remains increased for some days ('Compt. Rendus,' Mars, 1852), Dr. Budge adds the remark, that he had observed the same fact already in December, 1851. In another experiment, Dr. Budge destroyed the lumbar part of the spinal marrow, after which he observed a considerable decrease of temperature in the whole of the posterior part of the body. —*Froriep's Tagesber.*, No. 512, 1852.

Anatomy of the Male Mammary Gland. By Professor Luschka.

The *situation* of the *nipple* is by no means quite constant, although it is generally used as a fixed point in the physical examination of the chest. Among 60 cases, it was 44 times in the space between the fourth and fifth ribs, 6 times upon the fifth rib, 8 times on the fourth rib, twice between the fifth and sixth ribs. Not rarely it was different on both sides. The average distance from the margin of the sternum is about 4¾ inches, but it is sometimes half-an-inch less. Concerning the *contents* and *structure*, Luschka has frequently squeezed out from the nipple a fluid, clear like water, containing (1) molecular granules, (2) roundish globules, 0·004 to 0·006mm. in size (composed of small granules), and (3) spherical pale cells, as large as 0·012mm. with an excentrical nucleus, in form and appearance like the granular globules just mentioned. The areola and the nipple itself are endowed with *sebiparous* glands and numerous *papillæ*, as well simple as compound ones, in which the minute bloodvessels frequently form spiral slings, which might be mistaken for terminating slings of the nerve-tubes, but he could not find anything like Wagner's corpuscula tactus. As well in the nipple as in the areola Luschka met with many organic muscular fibres, such as Kölliker describes, in the female breast. The *parenchyma* of the male mammary gland is very scarce; it appears as a white fibrous mass, without showing any arrangement into lobules; it contains now and then a few small miliary vesicles, which are sometimes a little larger, and show more the form of a bag about 1·0mm. broad, and 2 to 4mm. long, ending in the nipple. By the microscope is recognised: 1. A *fibrous stroma*, formed by (*a*) *cellular tissue* as the principal component, which exhibits the different forms of development more numerously than in any other organ; (*b*) *elastic fibrillæ* in large number, without nuclei, unchanged by acetic acid: (*c*) *organic muscular fibres* in small number, with distinct nuclei. 2. The *glandular tissue* displays only few of the *glandular vesicles* which are so numerous in the female mamma: no regular acini are found, but sometimes a few of them (0·5 to 1·0mm. in size) are seen uniting by their tubular prolongation into a larger duct, which disappears in the nipple, now and then small excavations are found, seemingly formed by the combination of several vesicles. The thin lining membrane of these vesicles and excavations consists of polygonal epithelium, with a small granular nucleus; within their cavities, small, nearly round, globules (0·004 to 0·006mm.) are discovered, which appear composed of very small granules, scarcely changed by acetic acid, but easily dissolved by liquor potass, leaving only molecular granules; some of them are surrounded by a thin membrane, which seems

more or less protracted at the ends, and disappears to the eye by the addition of acetic acid; Luschka considers them to be an early form of development of intercellular fibres.—*Müller's Archives*, 1852, No. 4.

On the Reproduction of Nervous Substance, and on the Structure and Functions of the Spinal Ganglia. By Dr. A. Waller.

Dr. Waller, after having made many experiments on different animals, principally warm-blooded ones, of an early age, and frogs, considers himself entitled to the conclusion: "That the old fibres of a divided nerve never gain anew their original structure and function, and that the reproduction of nervous substance does not take place merely in the cicatrix itself, but also downwards into the terminating ramifications. The *old* fibres gradually waste, and after a month or later, *new* fibres are formed, which are pale and transparent, possess no double contour, present a very unequal diameter, being on the one place very thin, on the other, varicose, like the fibres of the spinal marrow. In the peripheral part of the glosso-pharyngeal nerve of a frog, three months after the section, their size was only about one-sixth to one-third of the original fibres; they resembled, therefore, much more the ramifications of the nerve in very young frogs. In the central part of the cut nerve the fibres remain unaltered. Concentrated acetic acid dissolves the membrane of the newly-formed fibres, leaving fusiform nuclei; the membranes of the original fibres are completely dissolved, no nuclei being left. The reproduction of fibres, and the return of function, proceed in the same proportion. Of great importance are Dr. Waller's experiments for the understanding of the *structure and function of the ganglia.* While, as he has previously shown, all motor nerves, separated from their cerebro-spinal centre, become entirely changed in their microscopic appearance, the peripheral part of the sensitive spinal nerves, the root of which is cut through between the spinal cord and the spinal ganglion, remains unaltered as long as the connexion with the ganglion is maintained. Ten or twelve days after having divided one or both of the roots of the second cervical nerve, he was enabled to make the following observations: 1. That part of the sensitive nerve which is situated between the place of division and the ganglion, is disorganized in the same manner as any dissected nerve in its peripheral end. 2. Tracing the disorganized fibres into the interior of the ganglion, they are seen mixed with normal fibres; the disorganized ones appear to pass into ganglionic globules, which are likewise altered, seeming to be deprived of their contents, and to consist merely of a thin, indistinct membrane. 3. The normal fibres appear to end by very thin filaments passing into normal ganglionic globules. 4. All the fibres originating within the ganglion are in their normal state. 5. The motor fibres are completely disorganized in the whole of the peripheral part of the nerve (no motion is produced by galvanism, or any other stimulus). 6. After having divided only the posterior root, all the fibres *below*, or on *the other side*, of the ganglion were normal. 7. After having divided the nerve *below* the ganglion, or after having cut out the ganglion, all the fibres in the peripheral part were disorganized. It is evident, from this, that the spinal ganglion acts as a nervous centre for the sensitive fibres, but not for the motor ones. Dr. W. promises to give soon more detailed observations, as well on the same subject as on the function of the nervus vagus and sympatheticus.—*Müller's Archives*, 1852, No. 4, p. 392.

On the Glands of the Mucous Membrane of the Human Stomach. By Dr. A. Ecker.

From the careful examination of the stomach of several suicides, Ecker gives the following statement concerning the gastric glands. In almost the whole of the stomach are merely simple cylindrical glands $\frac{1}{2}$ to $\frac{3}{4}'''$ long and $\frac{1}{60}'''$ thick, going in

a straight line through the mucous membrane, ending in a clublike swelling, very rarely exhibiting a division of the blind end. They contain round and angular cells, of a diameter of 0·017 to 0·020^{mm} with a nucleus composed of larger granules; towards the open end are seen more developed cells, towards the blind one, more nuclei and granular matter. At the *cardiac end*, besides these simple glands, other glandular follicles are situated, the blind end of which is divided and pouched; their contents are the same as just described, except that more fat-granules are seen towards the blind end. Near the *pyloric orifice* he constantly found, besides the simple, also *acinous glands*, deciding by this against Frerichs ('Wagner's Handworterbuch,' iii. 748) and Kolliker ('Mikroscop.-Anatom.' vol. ii. pp. 139 and 149), in favour of Bischoff ('Muller's Archives,' 1838, p. 515). We observe, therefore, no abrupt change in the structure of adjacent parts of the intestinal tube, but only a gradual one, single acinous glands being situated in the mucous membrane of the œsophagus, and a larger quantity of them in the duodenum.—*Henle's & Pfeufer's Zeitschrift, f. ration. Medicin.*, 1852, vol. ii. p. 213.)

Chemical Composition of the Uterine Fibres in Pregnancy. By Siegmund.

The point of interest in this communication is, that kreatin was obtained from the tissue of the gravid uterus. Formic and acetic acids were also present.—*Wurzburg Gesell. Verhandl.*, Band iii. Heft 1, p. 50.

The Crystals of Carbonate of Lime in Urinary Sediments. By G. Siegmund.

In the sediment often found in the urine of guinea-pigs, crystals, which chemical analysis prove to be carbonate of lime, are seen of the following forms: little rods, isolated or crossing each other at right angles, or grouped rosette-like, rods with nobby ends, and globules, two of which are often united by a rod. These last crystals are dumb-bells, and they lead the author into a discussion on the dumb-bell crystals of Golding Bird and others, from which he concludes that about dumb-bell crystals there is nothing *specific*—i. e., that it indicates no special chemical body, but may be formed by many substances. Returning to the dumb-bell and other crystals of the carbonate of lime in the urine of the guinea-pig and the horse, the author traces, with great care, all modifications to their fundamental form, the rhombohedron, and to various combinations of this. The typical form of dumb-bells is constituted by two opposed rhombohedra; the rod-shaped crystals (whose structure can be best made out when they are large) are formed by two rhombohedra joining in the direction of their main axes, and having the interspace filled up by new deposition, which goes on until the origin of the form may be undiscernible, and from the two rhombohedra a six-sided prism may arise. The mode of formation of these crystals and of the dumb-bells is, therefore, almost the same, and almost all the crystals of carbonate of lime can be thus referred to modifications of the rhomb. We must refer to this interesting paper for the full details, for the diagrams to illustrate the scheme, and for the representations of the various crystals as actually seen.—*Virchow's Archives*, Band iv. Heft 4, p. 505.

PATHOLOGY AND PRACTICE OF MEDICINE.

Contribution to the Pathological Physiology of Pneumonia. By Dr. G. ZIMMERMANN.

ZIMMERMANN considers the most minute observation of the natural course of acute disease to be the only way of elucidating the laws of these typical processes. Although he thinks those cases most fit for the purpose in which no remedies are employed, yet he does not at all exclude those in which the treatment is active. As an instance of how to observe, he describes the course of a case of pleuro-pneumonia, treated by repeated venesection, application of blisters, and internal administration of antiphlogistic remedies, in moderate doses. From the first day of the fully established disease, till after the return of convalescence, he has noted twice daily (at 8 A.M. as the time of remission, and between 5 and 6 P.M. as the time of exacerbation) the temperature under the tongue, the frequency and other qualities of the pulse, and the number of inspirations on almost every day, the quantity of urine secreted in 24 hours, its specific gravity, and the quantity of lithic acid contained in it. Of great interest are the *critical symptoms*, remarked on the 3rd and then again on the 9th day, after which we observe a steady progress to convalescence. The first note on the *temperature* was taken four hours after an attack of general vehement rigors, which the patient himself considered as the commencement of the disease; it was then as high as 103°10, which, being 4°14, over the normal warmth of that individual (98°96), makes Zimmermann inclined to conclude, that the disease had commenced before the occurrence of the rigors. Between the evening of the 2nd and the morning of the 3rd day, a decrease from 104°0 to 99·86 was observed, coincident with a general abatement of the constitutional symptoms of pyrexia, but on the evening of the 3rd day, the temperature was again as high as 105°80 and the physical examination exhibited the signs of hepatization, which had not been present in the morning. Between the evening of the 8th and the morning of the 9th day, the warmth had decreased from 106°16 to 99·50; at the same time all the other febrile symptoms disappeared almost completely, after they had reached a very high degree on the two previous days. It is interesting to remark, that although the frequency of the pulse did not exceed the normal average after that period, yet the temperature increased again to 102°20, and remained so for more than a fortnight longer. The entrance of the change on the 9th day, does not appear to Zimmermann as merely accidental, but he looks at this day as a critical one; and also the coincidence of several alvine evacuations effected by calomel seems important to him, as he thinks that the crisis may be promoted by our remedies, if their action takes place shortly before or on the beginning of a critical day. Concerning the *urine*, the quantity secreted in twenty-four hours was between 9040 and 29,030 grains; the minimum is noted on the 5th, the maximum on the 9th day, the average quantity of 24 hours before the 10th day, was 15,215 grains, with an average specific gravity of 1·0198; between the 10th and the 40th day, 16,510 grains, with 1·0208 specific gravity. The quantitative examination for *urea* was not made before the 8th day, on which 1081 grains were contained in the urine in 24 hours; on the 9th day, only 884 grains; and a few days later, not more than 400 grains (which is about the average quantity during health—*Lehmann*, Physiol. Chem., vol. i. p. 167). The quantity of *uric acid* was almost normal during the commencement of the disease, but after the 6th day it appeared considerably increased, and reached the maximum on the 9th day, when it amounted to 3770 grains in twenty-four hours (which is at least three times more than the average during health—*Lehmann*, l. c. p. 217). Zimmermann considers this increase of the uric acid and its salts, at the period of the critical change, as important for the doctrine of *crisis*, as he had opportunity to observe the same phenomenon, not only in pneumonia, but also in typhoid fever, ague, measles, &c. From three examinations of the blood obtained during the first seven days

in this case, and from his previous experience, Zimmermann considers as the most important changes in the *inflammatory blood*—that it is less coagulable than the blood in health; that the quantity of fibrine is augmented; that the coloured blood-globules are diminished in number, and possess an abnormal disposition for the formation of rolls; that the colourless globules are found in an increased proportion, and show, likewise, a tendency to join in groups.—*Prager Viertiljahrschrift*, 1852, vol. iv. p. 97.

Decennium Pathologicum: Contributions to the Pathology of Chronic Disease. By Thomas K. Chambers, M.D.

These eight papers are the beginning of a series, designed to test, by statistics, the accuracy of our generally received opinions concerning the effects of disease. The author has, as yet, not got beyond "Tuberculosis." The groundwork is the record of about 2500 fatal cases, of all kinds, preserved in the post-mortem books at St. George's Hospital. The first step was the construction of "an index, in which each morbid appearance observed is alphabetically enumerated, with a reference to the volume and page where it is found:" on the plan, we presume, of Giambattista Morgagni's *Index visorum in Cadaveribus*, in the Venice edition of the *Epistolæ de causis et sedibus morborum*. Then instances of various lesions were arranged in tables, under the head of the various viscera affected or the nature of the disease, and the index and tables submitted to mutual correction. The tables are not published, but simply a numerical enumeration of various facts which appear in them, of which, of course, only an outline can be here attempted.

The two first papers are devoted to statistics of the general mortality of the hospital, as a mean of comparison with those of particular diseases, in order that facts which appear in the history of the latter may be assigned to their due causes, whether those are the peculiarity of the malady, or of the locality in which it is observed.

The results are—1st. There is an excess of about $\frac{1}{36}$ in the male deaths throughout the country.

2nd. This excess is greater in hospitals, and amounts, at St. George's, to $\frac{44}{100}$, when accidents are excluded on both sides; and to $\frac{63}{100}$, accidents included.

3rd. This real excess is augmented in the general hospital reports by an excess in surgical male patients.

4th. The causes of the real excess are in a great measure of social, and not of physiological origin.

5th. The excess of accidental deaths is on the male side at all ages, but most in the middle period of life.

6th. The excess of deaths from other causes was, up to 20, slightly on the female side, but much less so than in the ordinary population of England. After 20, it was on the side of the males, and attained its maximum at 35.

In the third paper the prevalence of tuberculosis and the influences upon it of age and sex are discussed. In no less than twenty-five per cent. of the cases examined, tubercle was found. The next important point is the great preponderance of males in those affected. "In every 100 men there were more than 27, and in every 100 women not quite 22, affected with tuberculosis." The variations of this preponderance at different ages are then shown.

The fourth paper examines the seat of tuberculosis, and then the usual position of it in the lungs, when they are diseased. The idea of its preference for one lung over the other is held to be a fallacy dependent on the usual mode of examining living patients. Pneumonia, pneumothorax, and the conversion of tubercle into chalk are then examined. In the fifth and sixth papers, the location of tubercle in other parts besides the pulmonary organs is gone into with considerable length of detail. The prevalence of tubercle in the kidneys, much greater than had been represented by previous pathologists, calls forth remarks on the connexion between those organs and the skin, and the consequent importance of

guarding it from climatic influences equally with the lungs. Tuberculosis of the intestinal canal is shown to be almost entirely confined to the junior periods of life, and attention is therefore drawn to the different precautionary treatment of patients at various ages. This part of the subject is concluded by speculations on a connexion which appears, by their order in the tables, to exist between the liability of a part to tubercle and its degree of venosity; the inference being exactly the reverse of the doctrine professed by Rokitansky. In the seventh paper, the complication of tuberculosis with diseases of the heart is examined. The connexion is shown to be unusual, by a comparison of the frequency of cardiac lesions in tuberculous and non-tuberculous bodies, but still is much more common than observers of living patients only are aware of. In the post-mortem examination at St. George's, lesion of the heart occurred in seven per cent. of cases of pulmonary tubercle. In the eighth paper, the complications of non-tubercular diseases of the brain, such as softening, inflammation, effusion, &c., with pulmonary tuberculosis, are examined in the same manner as heart-disease was in the former one.

The eighth paper inquires into the connexion of tuberculosis and cerebral disease, both in respect of cases where tubercle existed in the brain and where it did not. Omitting the numbers, the conclusions of the observation of the 2161 cases examined at St. George's Hospital in ten years are—

1st. That the secondary consequences of tubercle in the brain were the same, whether the tubercle be in the substance or membranes.

2nd. That the symptoms of these secondary consequences were, in the case of inflammatory action, pretty uniform, but otherwise obscure and variable.

3rd. That independent of tubercular deposit, idiopathic inflammatory conditions of the meninges were most common in the tubercular diathesis—nay, almost peculiar to it.

4th. That the same diathesis disposed also to softening of the cerebral substance, probably of an inflammatory character; but that other diseases had nearly as great a tendency to produce softening, whether truly inflammatory or not is unknown.

5th. That serous effusion on the brain was less usual in tuberculous persons than in others.—*Medical Times and Gazette*, August, December, 1852.

The Respiration in Pressure on the Brain. By Dr. Landgraf.

Dr. Landgraf calls attention to the state of the respiration, in cases of cerebral pressure. It is frequently not stertorous and laboured, as described in books, till the agony; but it is interrupted, that is to say, after from six to twelve tranquil and easy respirations, a long pause ensues. The author details cases in proof of the existence and diagnostic value of this sign.—*Deutsche Klinik*, 1852, p. 39.

Temporary Albuminuria. By Dr. Begbie.

Dr. J. W. Begbie alludes to the phenomenon of albuminuria in the following diseases:

1. *Scarlatina Simplex.*—He confirms his former statement that about the period of desquamation, albumen can *almost* always be found; its presence is associated with renal epithelium, but not with casts of tubes.

2. *Cholera.*

3. *Erysipelas.*—Usually at resolution or during convalescence. Its presence is not constant, nor its quantity great.

The albuminuria in these three cases is called *desquamative.*

4. *Scarlatinal Dropsy.*—The albuminuria may, or may not, be temporary—blood exudation-corpuscles, and casts of tubes, accompany it.

The albuminuria in this case is termed *inflammatory.*

5. *Pneumonia,* at the period of resolution, in almost all cases.

6. *Typhus* and *Typhus Abdominalis* (typhoid).—From a consideration of the period when the albumen is observed in these last-named diseases, Dr. Begbie terms it *critical albuminuria.*—*Monthly Journal*, Oct. 1852.

On Albumen in the Urine of Various Diseases.

HELLER asserts that albumen is present in the urine in all kidney-lesions, though sometimes in small quantity, and that it exists in many other diseases, and often in greater amount.

1. *Pneumonia* and *Tuberculosis acuta.*—At the commencement of exudation, while yet the chloride in the urine is in undiminished quantity, no albumen can be found. As exudation increases, and as the chloride in the urine diminishes, a very small quantity of albumen appears, and continues for a long time. This appearance is not constant, but is very frequent. The greater the albumen, and the less the chloride in the urine, so much the worse is the prognosis.

2. *Pleurisy.*—Albumen does not appear so frequently, even when the chloride is much diminished. In the period of absorption it sometimes occurs, and is attended with carbonate and hydrothionate of ammonia.

3. *Acute Liver-Affections.*—In chronic or subacute inflammations, where the chloride of the urine is diminished, albumen appears as in pneumonia.

4. *Pericarditis* and *Endocarditis.*—In the first case albumen sometimes occurs; in the last, very seldom, even when the chloride is much diminished.

5. *Peritonitis.*—Albumen is frequently found, and continues sometimes long after the customary amount of chloride has reappeared, and morbus Brightii is then, perhaps, left.

6. *Metritis* and *Eclampsia Puerperalis.*—As in peritonitis.

7. *Cholera.*—More or less albumen.—*Archiv. für Pathol. Chem.*, Band i. Heft 8.

On Cirrhosis of the Liver. By M. MONNERET.

M. MONNERET, believing that the term cirrhosis has been applied to very different pathological conditions, endeavours in these papers to speak of it with some precision, while relating the cases of the disease which have come under his own observation. He defines cirrhosis as a chronic and apyretic affection of the liver, characterized by more or less impediments to the hepatic portal circulation, which leads to peritoneal effusion, dilatation of the collateral veins, and often to œdema of the extremities, and hæmorrhages from the various mucous surfaces.

The *anatomical changes* which almost always accompany these symptoms are induration and atrophy of the entire tissue of the gland, or of its vascular portion, with the yellow change of tissue, whether granular or not. The retraction of the hepatic substance gives rise to the diminished size of the organ, and at the same time that the portal veins become less visible, the yellow portion continues to predominate over the other, until it entirely supersedes it. The thickening of Glisson's capsule and the serous layer that lines the liver completes the anatomical character.

M. Monneret's memoir is based upon twenty-four cases, in fourteen of which autopsies were performed, this being the entire number of cases he has been able to meet with during the ten years his attention has been directed to the subject. He has compared these cases with forty others of the various lesions of the organ, as also with fifty cases of disease of the heart, in which the condition of the liver was examined.

He has taken great pains in the measurement of the liver, by means of plessimetry, having accurately measured in this way 100 patients. In a healthy man lying in the horizontal position, the hepatic dullness commences four centimetres (about 1½ inch) below the right nipple, and terminates at the edge of the ribs, which forms a tolerably exact natural inferior boundary. At the medium line, it is placed behind the scaphoid cartilage, passing a little towards the upper part of the epigastrium. Posteriorly and laterally it ceases at the level of the ribs. The following figures indicate the normal distances which separate the upper line of hepatic dullness from the level of the ribs. In thirty-one cases its mean height at the *median line* was 5·62 centimetres; its minimum 1·5, and its maximum 9·5. To the right *nipple* the mean was 12·64 centimetres, the minimum 7·8,

and the maximum 18. In the *axillary* region, the mean was 10·57, the minimum 7·3, and the maximum 13. In the *scapular* region, the mean was 9·11, the maximum 14. In twenty-five cases the hepatic dullness commenced at four centimetres below the nipple. The *thoracic vibration*, perceptible to the hand, while the patient counts with a loud voice, extends three or four centimetres below the upper limit of hepatic dullness. In cirrhosis the normal limits of hepatic dullness have never been found exceeded. In some cases they are scarcely diminished, while in nine have they been so by more than five centimetres. The meteoric state of the intestines renders exploration sometimes difficult by pressing the liver into the thoracic cavity: but when authors speak of hepatic hypertrophy they confound other lesions with cirrhosis.

The *peritoneal effusion* is of very slow occurrence, fluctuation long continuing obscure, and the patient often not being aware of tumefaction of the abdomen. The progress of the dropsy is gradual, and there is not observed those alternations of increase and diminution seen in dropsy arising from hepatic congestion, whether connected with disease of the heart or other lesions. The fact of the dropsy becoming established before anasarca of the extremities, has been too much generalized. The integuments of the abdomen becoming infiltrated sooner than can be explained by the abdominal distension is explicable by the obstruction to the venous circulation.

In like manner the *dilatation of the veins* of the abdominal and thoracic parietes may become considerable before any notable effusion occurs. The most delicate *capillaries* undergo dilatation, so that their elegant arborescence becomes perfectly visible, and that by no means always when distension is greatest. These facts are explicable by the obstructed state of the portal circulation. In some cases, however, no such dilatation and inosculation of veins is present, ascites existing alone. In this point of view, it is interesting to remark that in five out of ten of Dr. Hillaret's cases of portal phlebitis there was no effusion. It is probable that in some of these, as well as in some of the cases of cirrhosis, the obstruction has only been partial, and hence the absence of some of the usual symptoms.

Not only, however, is the hepatic circulation thus disturbed in cirrhosis, but in all probability the *composition of the blood* has undergone change, giving rise to the *hæmorrhages* which are of such frequent occurrence. Epistaxis, slight in quantity, is the form that M. Monneret has usually met with; in some cases the stools have been tinged with blood.

We cannot abstract the details of the eleven *autopsies* M. Monneret furnishes an account of; but may advert to his summary of the most common lesions. 1. The liver is sometimes diminished by a third or one half its size. 2. Its surface presents more or less prominent lobules, separated by whitish furrows, the normal disposition of the hepatic structure being exaggerated. 3. The capsule of Glisson is thickened, whitish or opaque, more close and resisting, and intimately adherent. 4. This capsule is found in a hypertrophied state, in the interior of the parenchyma, as whitish lines, enclosing the hepatic lobules and sometimes yellow granules. 5. The change in the proportion of the two substances of the liver has long been admitted as a characteristic of cirrhosis; but while acknowledging the convenience of the expressions red or vascular, and yellow or bilious portions, and believing the affection is one which obstructs the circulation in the vena porta, Monneret doubts the correctness of these anatomical statements. M. Lereboullet believes in the conversion of the bilious into fatty cells, and Monneret has always found by the microscope that a large quantity of fat incrusted the biliary cells. He believes, however, that this fatty transformation itself is dependent upon the atrophy of some element of the parenchyma. 6. The extreme frequency of perihepatic peritonitis is of importance in the anatomical history of cirrhosis; for it may be asked whether this phlegmasia induces induration of the proper membrane of the liver, the loss of extensibility of which may be the cause of the hepatic retraction. 7. The degree of induration of the liver varies, being in some cases

comparable to scirrhus, and apparently due to the predominance of the cellulo-fibrous portion. 8. There is great dryness of tissue from paucity of blood. 9. The alteration may occupy an entire lobe or even the entire organ; but it may do this in very different degrees. 10. As a negative character worthy of note, it may be mentioned that there are no lesions of the bile ducts, and the bile is apparently normal. 11. In several cases a new circulation has been found established in the fibro-cellular partitions of the lobules, a brilliant arterial network being distinctly visible. This may be regarded as a supplementary circulation of the hepatic artery, it having been observed in cases in which the vena porta was entirely or partially obstructed.

In respect to the *causes* of this affection, the habitual excessive use of alcohol is undoubtedly one; but in other cases bad and insufficient diet is alone discoverable. These circumstances explain the frequency of disease of the alimentary canal, which is observed in cirrhosis. The frequency with which inflammatory disease of the liver has preceded this condition is undoubted: but whether the thickened state of the capsule be an extension of this, or one of the lesions accompanying organic atrophy, is doubtful. Inflammation is not essential, as in certain cases it has not prevailed. The congestions of the liver which are so frequently seen in disease of the heart are not, as has been stated, first stages of this affection. Their effect is to lead to dilatation of vessels, while cirrhosis leads to their obliteration. In cirrhosis the yellow secreting tissue, formed of biliary cells, and yellow granules, is not hypertrophied, and only becomes more visible and prominent from the atrophy of the portal and vascular system. In hypertrophy the functional activity gives rise to jaundice, but not to obstruction of the circulation; while in active congestion and phlegmasia, even when slight, characteristic symptoms are present, as increase in size, tenderness, irregular fever, fibrinous blood, and icteric urine.

For the *treatment* of so fatal a disease M. Monneret has little to recommend. At least temporary benefit is sometimes derivable from alterative doses of blue pill, combined with Vichy or soda water, and alkaline or sulphureous baths. The diarrhœa and vomiting so obstinate in some of these cases are best treated by large doses of bismuth.—*Archives Générales*, tom. xxix. 385, & xxx. 56.

Ichthyosis Cornea. By H. Muller.

The author describes fully a case which, in point of severity, though not in respect of hereditariness, stands near the cases of the family Lambert. The crusts, on section, were found to be composed of a system of concentric rings, made up solely of epidermic-cells; between the rings, epidermis was irregularly arranged. The whole structure resembled Gustav. Simon's representation of a section of a wart, but the rings were not joined by the cuticle sheathing the papilla, and the masses lying between the rings by the cuticle formed by the parts between the papillæ, as in the case of warts, but each ring-system corresponded to a hair-bulb or to the duct of a sebaceous gland; spiral ducts of sebaceous glands pierced the mass. Ichthyosis, however, may be of various kinds, and especially in elephantiasis the papillæ are chiefly engaged, are long, and hardened and sheathed with abundant cuticle. The author proceeds to make some general remarks on ichthyosis and abnormal cuticular development, from which it is to be inferred that he believes ichthyosis may have, so to speak, various points of departure, and may be connected with hypertrophied papillæ, with altered hair-bulbs or sebaceous follicles, or even with degenerated sweat-glands.—*Wurzburg Gesell. Verhand.*, Band iii. Heft 1, p. 40.

Leucocythemia. By Dr. Hewson.

Charles Robinson, aged 17; never had ague, but had been in miasmatic districts; came under the care of the author; he was anemic, and had œdema of the lower

extremities, and diarrhœa, and on one occasion epistaxis. The spleen, marked out by percussion, measured 8½ inches by 8 inches. The blood showed "a great redundancy of white corpuscles." He was treated with iron and quinine, to which mercury and nitro-hydrochloric acid, were added, for a short time. In five weeks he was cured: the spleen was of its normal size, and the blood was healthy. When he was seen three months afterwards, however, the colourless corpuscles were found to be too numerous, although he appeared in perfect health. The author (who is a grandson of Hewson, and who refers, with pardonable pride, to his ancestor's well known opinion on the functions of the spleen) has examined numbers of patients with splenic enlargement from intermittents, without detecting any leukæmia.—*Amer. Jour. of Med. Science*, Oct. 1852.

The Uræmic Hypothesis of Frerichs. By G. Zimmermann.

The author criticizes with great keenness the late statement of Frerichs, that the phenomena of the so-called urinary intoxication are owing to decomposed urea. The arguments against this view are chiefly drawn from an analysis of Frerichs's own observations, which are shown to be very incomplete. The following table gives the opinions of the one, and the criticisms of the other:—

Frerichs.	Zimmermann.
No bad consequences result from the injection of urea into the blood; the contrary results obtained by others, arose from the urine being unfiltered and loaded with epithelium.	The explanation is insufficient. Besides, in retention of urine, in the blood of Bright's disease, the blood is not, probably, normal; between urine injected and urine retained there may be no analogy.
Urea, at page 113, is said to be a harmless substance.	Urea, at page 50, is said to injure the cerebral organs.
The urea is decomposed by a ferment, which is generated more easily in febrile than in apyretic conditions.	The existence of the ferment is unproved. If it occurs, may not the ferment itself be the cause of the symptoms?
The urea is decomposed into carbonate of ammonia, as proved by the presence of ammonia in the breath, and by examination of the blood.	The blood of perfectly sound individuals may contain an ammoniacal salt, which passes off with the halitus sanguinis, and is easily detected by the fumes formed with hydrochloric acid. It is probable that ammonia is constantly given off through the skin (Gerlach and Schottin), lungs, and kidneys. The breath, as Marchand and Lehmann have shown, and as anybody may prove, often contains ammonia.
Carbonate of ammonia injected into the veins of dogs produces coma and convulsions.	The dogs were never killed with it. Would men be equally affected? Carbonate of ammonia given internally in large doses has not this effect.
Clinical experience confirms the hypothesis.	In Frerichs's book, blood was drawn three times in Bright's disease, but carbonate of ammonia was never sought for. The ammonia in the lung-exhalation was proved only in three cases.

Zimmermann proceeds to adduce other similar arguments, in order to show what he considers the incompleteness of the evidence brought forward in support of this ingenious hypothesis.—*Deutsche Klinik*, No. 37.

Obliteration of the Renal Veins in some Diseases of the Kidney, and especially Albuminous Nephritis. By M. Leudet.

M. Leudet, having met with two cases of albuminous nephritis, in which the renal veins were obliterated, has examined the various recorded examples of this lesion, with the view of ascertaining how far it stands in relation of cause and effect to this alteration of texture in the kidney; and whether it is also found in other affections of this organ. This latter point is answered affirmatively; for Rayer relates a case of such obstruction in hypertrophy of the organ. Rayer and Dance met examples of it in nephritis; while several cases are on record of propagation of uterine phlebitis to the renal veins. Still, it is in relation to "albuminous nephritis" that most examples have been observed. Rayer, Stokes, Peacock, and Delarnelle, have each related cases, and M. Leudet now adds two others. Yet even in this form of renal disease the lesion is rare, as, notwithstanding M. Leudet's attention has been directed to the subject for several years, these are the only cases he has met with.

Whether such lesion of the veins be cause or effect of the structural change, or a mere coincidence, is a doubtful point. Frerichs, however, regards the obstruction so produced as one of the causes of albuminous urine, and a source of a more or less rapid disorganization of the kidney. He has performed various experiments on animals, in which, by obstruction or compression of these vessels, he has been able to render urine speedily albuminous; and he refers to similar results obtained by Robinson and H. Meyer. To draw safe conclusions from these, however, the kidneys should be examined at a remote epoch; for, although ligature of the renal veins may readily give rise to albuminous urine, it is quite another question whether it will give rise to the disorganization of the kidney termed Bright's disease.—*Gazette Medicale*, 1852, No. 44, pp. 681.

SURGERY.

Statistical Account of the Ligature of the Principal Arteries. By M. Roux.

At a recent meeting of the Surgical Society, M. Roux detailed the results which have attended his numerous applications of the ligature to the large arteries. From 1808 to the present time, he has ligatured 82; of these, 33 were for true or spontaneous aneurism. These are distributed as follows:—1 of the popliteal artery for aneurism by the old mode;—46 of the femoral artery—of these, 27 have been for popliteal aneurism by the Hunterian operation, 3 for femoral aneurism, 7 for wounds and primary hæmorrhage, 7 for secondary hæmorrhage, and 2 for fungous tumours of the tibia;—20 of the brachial artery—of these, 10 were for false consecutive aneurism at the bend of the arm, 6 for arterio venous aneurism, 1 for spontaneous ulnar aneurism, 2 for secondary hæmorrhage, and 1 for fungous tumour of the radius;—6 of the common carotid artery—1 of these was for fungous tumour of the orbit, 2 were for wounds of the face or neck, and 3 preventive ligatures in operations;—4 of the axillary artery immediately below the clavicle—1 was for true, 1 for false aneurism, and 2 for hæmorrhage after amputation at the shoulder-joint;—3 of the subclavian artery for secondary hæmorrhage;—2 of the external iliac for hæmorrhage consecutive to ligature of the femoral. Of these ligatures, 16 were applied by the old method, and 66 by the Hunterian.

The entire number of *aneurisms* so treated has been 49—viz., 33 true, 10 false, and 6 arterio-venous—of this number, all but two (which were successful) were treated by Hunter's operation. Brasdor's has never been performed by M. Roux. Of the 33 true aneurisms, 31 occurred in men and 2 in women. In 28 of the cases, the age varied from 27 to 40, and the oldest patient was aged 59.

Of the 33 *true* aneurisms, 23 were cured, and 10 were treated without success. In 2 cases superficial, and in 2 complete gangrene occurred. In 4, secondary

hæmorrhage took place—viz., on the 4th, 22nd, 34th, and 50th days. The 10 cases of *false* aneurism, all arising from venesection, were all cured. Venesection also gave rise to the 6 cases of *arterio-venous* aneurism for which the brachial artery was tied, in 4 with success, while in 2 hæmorrhage and gangrene necessitated amputation.—*L'Union Médicale*, 1852, No. 124.

On Medullary Sarcoma, principally that of the Membrum Virile and its neighbourhood. By Professor WUTZER.

As the principal seat of medullary sarcoma, Wutzer considers the interstitial cellular tissue between the muscles; rarely he found it originating from the nerves. Amongst the parenchymatous intestines, it was most frequently in the testicles, where he observed the occurrence of *primary* medullary sarcoma; from these he noticed in one case its spreading to the *membrum virile* ('Deutsche Klinik,' 1841, p. 160); but he doubts, whether it ever *primarily* occurs in the latter organ, *epithelial cancer* being, according to his observation, the principal malignant affection by which it is attacked. Concerning the question of the removal of the affected organ by surgical operations, Wutzer considers the latter as prolonging life, if instituted at an early stage; as deleterious, if at a late stage, when secondary affections are present.—*Illustrirte Medic. Zeitung, Dr. G. Rubner, Muenchen*, 1852, vol. i.

The various forms of Inflammation of the Joints. By DR. FÜHRER.

AFTER some introductory remarks, the author states that the anatomical division of the joint affections is into those which attack the synovial capsule, the other coverings of the bone, and the bone itself.

1. *Rheumatic joint inflammation.*—[By this term the author does not imply the simple joint-rheumatism, which is attended only with slight injection of the synovial membrane, increased exudation of serum, and a little velvety appearance of the cartilages.]

(*a*) Acute form.—Rapid pus-formation; plastic exudation on an intensely-injected synovial membrane; softening of the capsule, reaching even to rapid perforation, and passage of pus between the muscles and fascia; sometimes periostitis.

(*b*) Chronic form—Leads essentially to necrosis of the joint, and fistulous communications, the joint contains grayish-black, bloody pus, and the capsule is covered with exudation; the internal ligaments are destroyed; the bones, as far as they reach into the joints, are discoloured, necrosed from without, often fractured. Externally is a callous thickening of the outer capsule, and infiltration of the neighbouring parts with exudation passing into fibroid tissue.

2. *Arthritic joint-inflammation.*—It is characterized by exudation in the spongy bone substance, by rarefaction, and mollities; lastly, in the third period, by shrinking, atrophy, and disfigurement of the ends of the bones.

The chronic form is much more frequent than the acute; the spongy substance becomes infiltrated with oily marrow, and later with yellow, hardened fat. There is no effusion in the joint, and no external swelling. As the bony substance becomes absorbed, fresh layers of bone are formed around; some of the ligaments are ossified; lamellæ of bone form under the synovial membrane, and osteophytes under the periosteum. A peculiar appearance are the "corpuscula mobilia," which arise, through fibroid thickening of the synovial villi at their free ends and detachment of the same; through ossification of the same and detachment; through division of pieces of cartilage following fissures. These last are often perfectly free, or connected only by a fibrous connexion. The *malum coxæ senile* is the commencement of the process occurring in the hip. In this disease the cavities of the joint are deepened and widened by the previous mollities; the surface of the joint denuded of cartilage; the ends of the bones covered with an irregular

ivory-like cortex; they are also flattened, surrounded with stalactites; the neck of the thigh bone is very much shortened and pressed in, so that the head of the femur is placed on the trochanter. The synovial membrane is dry, and causes often croaking on movement; the eburnation may occur in the depths of the bone-ends, and the abnormal ossification implicates sometimes the surrounding soft parts, and even the muscles.

3. *Podagraic Joint-inflammation.*—Both acute and chronic forms are little known. The former occurs exclusively in the small bones of the hands and feet: the ligaments and cartilages are unaltered; the faces of the joints are beset with a chalky coating; little chalk-formations are under the periosteum, and in the joint is a thick grumous fluid, which contains uric acid and urates. The neighbouring bursæ are also sometimes filled with a like fluid. The chronic form appears in anomalous gout, and, occurring in the hip-joint, is one of the forms of the *malum coxæ senile;* there is not, however, previous swelling and softening of the spongy substance of the bone, but pure "incalcination" and thickening with atrophy.

4. *Fungous Joint-inflammation*—(one form of tumor albus)—appears preferably in the knee-joint, and in the small joints of the hands and feet; is painless, insidious, and very chronic. The joint swells; the synovial membrane and the surfaces of the cartilages are thickly covered with grey-red, soft granulations. Similar fungosities grow through the chronically inflamed periosteum; the surrounding parts, fascia, muscles, &c., are partly infiltrated with exudation, and partly changed into a callous tissue, with much fat.

5. *The Tuberculous Joint-inflammation*—(one form of tumor albus).—The cartilages are penetrated with fistulæ, which penetrate into the bone substance, and communicate there with abscesses. When pus has entered the joint, the surface of the cartilages becomes softened, as if macerated; the diseased epi- and apophyses are strewed with little masses of shining exudation, or with yellow cheesy masses.

6. *The acute Empyema of Joints.*—Is an affection of the synovial membrane, passing rapidly into superficial caries. It occurs in puerperal diseases, but also often during pregnancy, after phlebitis and pyæmia, in the desquamative period of scarlet fever, in typhus, &c. The varieties are illustrated by cases.—*Virchow's Archiv.*, Band v. Heft 1.

Hernia Foraminis Ovalis. By Dr. R. Fischer.

Of an interesting paper on this subject, we can give only the following results. After describing the anatomy of the canalis obturatorius, Dr. Fischer mentions four passages, through which the hernia might occur. 1. Between the ramus horizontalis ossis pubis and the superior margin of the anterior portion of the muscul. obtur. extern., which passage is marked by the ramus abduc. posterior of the nervus obturator. 2. Between the anterior and the middle portion of the musc. obtur. extern., through the same opening with the ramus adduc. poster. of the nerv. obtur. 3. Between the membrana obtur. extern. and intern. following the decourse of the third branch of the nervus obturator. 4. Between the membran. obtur. extern. and the incisura acetabuli. Dr. Fischer himself, however, considers the occurrence of a hernia through the two last passages as scarcely possible. The *diagnosis* has been to him as difficult as to others (on account of the deep situation under a large layer of fat and the muscul. pectinæus), the principal symptoms being furnished by the percussion of that region, by the disturbance in the function of the nerv. obturat. and in the action of the bowels. Concerning the *statistics*, Dr. Fischer observes, that it is met with very rarely in children, and more frequently in women than in men—on account of the anatomical construction of the parts.—*Henle & Pfeufer's Zeitschrift für Ration. Medicin.*, 1852, Band x. p. 246.

Abscess, &c. in Bone. By Henry Lee, Esq.

The author details a case of abscess in the head of the tibia, cured by trephining, and one of necrosis of the femur treated in the same way. In reference to these cases and others previously detailed, it is stated that long-continued pain in bone may arise from a variety of conditions, and that the chronic irritation which precedes the deposit of new lime may depend, among other causes: 1. Upon the formation of pus within bone; 2. Upon solid deposit from mercurial or syphilitic poisoning; 3. Upon tubercular deposit; 4. Upon necrosis of the cancellated structure. In all these cases the remedy is trephining.—*London Journal of Medicine,* Oct. 1852, p. 884.

MIDWIFERY, &c.

Extirpation of a Mesenteric Tumour, simulating Ovarian Disease. By Dr. Buckner.

The case having been diagnosed as ovarian, and operation decided on, an incision nine inches long was carried from umbilicus to pubes; the tumour was then found to be not ovarian, but situated "*in the mesentery,* between the laminæ of the peritoneum, and surrounded by the small intestine." The operation was proceeded with, the tumour was dissected out, and the superior mesenteric artery, and other smaller arteries, tied. The patient recovered, and in spite of the great separation of the mesentery from the intestine, no apparent bad consequences of any kind ensued.—*American Journal of Medical Science,* Oct. 1852.

On the Effect of Prolonged Horizontal Posture in the production of the great mortality in Foundling Hospitals. By M. Hervieux.

M. Hervieux observes, that persons visiting the *crèche* of the Paris Foundling Hospital, admire the exquisite cleanliness, free ventilation, and mild temperature of that vast apartment. Still, of about 4000 infants annually admitted, about 3000, i. e. 75 per cent., die; and to explain this fearful mortality, the impoverishment of the blood of these victims of debauchery and poverty, their over-crowding, and the insufficiency of their nursing, have been referred to. All these have something to do with the result; but a chief cause of its production, hitherto overlooked, is the too prolonged maintenance of the horizontal posture. Each child is taken up, fed, and changed four times daily, and again at night, when it cries. Suppose this operation is performed six times on an average, as it only occupies about twenty minutes, the infant is lying on its back for twenty-two out of the twenty-four hours, quite unable at this age to change its position. Motion and exercise are essential to the well-being of the infant, and its proper place is its nurse's bosom, the warmth of which is imparted to it.

The children of the *crèche* die, in fact, of cold and hunger. Owing to the continuance of the horizontal posture, the temperature of the body becomes lowered, the limbs chilled, the circulation languid, and the respiration embarrassed. All the principal functions languish, the skin becomes indurated, and visceral congestions take place. Some of the children perish from sclerema, some from the so-called pnuemonias, which are only sanguineous stases, and others from various serous effusions or hæmorrhages. The definitive cause of all these disordered conditions is *cold,* not cold engendered by the diminished temperature of the surrounding medium, but cold resulting from their prolonged immovability.

We have also to inquire whether feeding infants four, six, or even eight times a day is sufficient. Books tell us that they should only be suckled at regular intervals, every three or four, or sometimes two hours; but any one practically acquainted with the rearing of young infants, must see the fallacy of this. In fact, they suck some thirty or forty times a day, absorbing, according to the calcu-

lations of Guillot and Lamperière, from three to four pints of milk. This suits them admirably, for in the first two or three years they have to acquire one-half the height and weight they will gain during the rest of their lives; and the limiting them to the periods and quantities suitable for older subjects is unphysiological and mischievous. It has been said that this so-called excess of food gives rise to the gastro-enteric affections, so frequently met with at this period of life; but, in fact, such diseases are not met with in private practice, either in the infants of the rich or of the poor, who are often so inordinately suckled, while the body of every child brought from the hospital exhibits more or less intense signs of acute or chronic gastro-enteritis. The practice of bringing up the children by hand has been assigned as a cause of the great mortality: but nothing similar to it is found among the children so brought up in the worst parts of Paris, where they, however, get abundance of milk and good nursing. At present the eighty-four infants at the *crèche* have only nine nurses and two night nurses to attend to them; while M. Hervieux considers that one woman cannot pay suitable attention to more than two infants.—*L'Union Médicale*, 1852, Nos. 139, 140.

Double Uterus. By Dr. Kelly.

An interesting and complete case of this kind is recorded by Dr. Kelly; there were two vaginæ (each had had its hymen), two uteri, Fallopian tubes, ovaries, &c. Reference is given to other cases.—*American Journal*, Oct. 1852.

Conversion of Arm-Presentation into Natural Labour. By Mr. Mayne.

In a case of arm-presentation, in which turning was impossible, Mr. Mayne pushed the hand and arm up above the pubic portion of the pelvis, and held them there for an hour and a quarter. A few minutes after withdrawal of the hand, the head was found to be descending naturally, and delivery occurred without further accident. Mr. Mayne is sure that this was a true case of arm presentation, and not merely of the descent of the arm with the head. In this case it was the left arm; the back of the child was to the abdomen of the woman, and the head rested on her right ilium.—*London Journal of Medicine*, Oct. 1852.

Case of Hermaphrodism. By Dr. Gross.

The author being called to a child, presumed to be a girl, three years old, found a small clitoris, natural nymphæ, large labia, containing each a well-formed testis, no vagina, and no penis. As sexual congress would manifestly be always impossible, the author deemed it advisable to castrate. A question then arises as to whether the operation was justifiable—the author, of course, takes the affirmative side.—*Amer. Journ. of Med. Science*, Oct. 1851.

MATERIA MEDICA AND THERAPEUTICS.

Remarks on Hæmospasia.—By Dr. T. Vogel.

Dr. Vogel strongly recommends as an important remedy the *hæmospastic apparatus* (ventouse monstre—Schroepfstiefel) invented by Dr. Junod, and improved by Dr. Ficinus, of Dresden. The intention is to expose the surface of the body, or a part of it, commonly the leg, to condensed or rarefied air. The apparatus for the lower extremity simply consists in a box of latten-plate, open on the top for the

introduction of the limb; around this opening a conical piece of vulcanized india-rubber is fastened, which is on its upper end slightly narrower than the thinnest part of the thigh it is to surround. Through a small opening in the box, by means of a simple air-pump, the air is rarefied to about $\frac{1}{10}$ or $\frac{1}{8}$, or even $\frac{1}{3}$ of its original normal density, the degree of rarefaction being easily ascertained by a *manometer* screwed into another little hole in the instrument. After the limb has remained for about half an hour under the influence of the rarefied air, it appears more or less swollen and hard, generally without much pain, sometimes showing slight extravasation of blood, if the rarefaction had been produced suddenly, or carried to a considerable degree. This tumefaction disappears gradually (within 8 to 24 hours). By measuring the limb before and after the application of the apparatus, we can estimate the increase of the quantity of blood which is accumulated in it, in consequence of the exposure to the rarefied air. An increase of the volume amounting, for instance, to forty-seven cubic inches, would indicate an increase of thirty ounces of blood in the swollen leg, and a diminution of the same weight in the quantity of blood circulating in the other part of the body. The effect of *hæmospasia* must be therefore the same as that of *venesection*, if only a temporary collapsus is requested; bleeding must be preferable where an actual loss of blood appears desirable, but in that great number of cases, which show the phenomena of local congestion or inflammation, in non-plethoric, or even oligæmic individuals, the actual loss of blood must do much harm, although a temporary detraction of it to a distant organ may be beneficial; and for such cases the use of rarefied air, by a convenient apparatus, like that of Junod, can be highly recommended. The disappearance of swelling and redness in hyperhæmical and inflammatory affections of external organs, for instance of the conjunctiva palpebrarum et bulbi, or in erysipelas faciei, is often striking. The operation in pneumonia and pleuro-pneumonia appeared very valuable to Dr. Vogel; the relief of the dyspnœa, the restlessness, and pain, by every application of the apparatus, was as great as that observed after venesection; it is true that the distressing symptoms generally make again their appearance after some time, but this is not less the case after venesection, although perhaps not quite so quickly; by repeated application, however, the same beneficial effect may be obtained without the disadvantages of the real loss of blood. It will be easy after this, for every one to find the cases in which the one remedy is more suitable than the other; for internal hæmorrhage, however, and for organic diseases of the heart, it is particularly recommended by Dr. Vogel. A well-made apparatus may be had as well at Dresden as at Giessen, for 2*l*.—*Illustrirte Medic. Zeitung, Dr. Rubner, Muenchen*, 1852, No. 1.

Remarks on Paracentesis Thoracis, and Suggestion of a new Instrument for this operation. By Dr. Wintrich.

From the experience gained as well in his own practice as in that of others, Dr. Wintrich speaks highly in favour of the paracentesis thoracis, and ascribes the want of confidence placed in it by so many medical men to the imperfection of the instruments used, and to the insufficient manner in which important points connected with every case are generally taken into consideration. As indicating the operation, Dr. Wintrich considers: 1. A high degree of dyspnœa produced by sudden exudation of liquid matter. 2. Gradual accumulation of the exudation to such a degree, that suffocation is threatening, or death from pressure on vital organs. 3. Insufficiency of the other remedies to effect resorption.

Dr. Wintrich cautions against mistaking the high degree of dyspnœa arising from accessory pleuritis, flatulence, &c. (in cases of slight exudation), as caused by a considerable increase of exudation, and as indicating paracentesis. He further remarks, that the operation should never be performed after the diseased side has become perfectly immovable (by paralysis of the muscles or by rigidity from old age), as the fluid under such circumstances cannot escape without being replaced by air

entering from without, the effects of which is not to be computed beforehand. The more moveable and elastic on the other side the wall of the thorax (short duration of the disease, young age), the greater is, cæteris paribus, the indication for the operation, provided we have proved (by the exploring needle) the exudation to be of a liquid nature. From the elasticity of the wall must also depend the quantity of fluid we may evacuate at once, and the space of time within which this may be done. The attributes which Dr. Wintrich considers necessary in an instrument used for this purpose, and which he finds combined in the one, recommended by him, are, that the entrance of air may be prevented, that the fluid can be made to flow off in a large stream or in a thin one, or even drop by drop; that in case of obstruction of the opening (by flakes, &c.), the impediment may be easily removed without the instrument being taken out.

Dr. Wintrich's instrument consists of two flat, narrow silver tubes, with sharp brims; the narrow one ends in front by a trocar-point, behind which is a large oval hole leading into the cavity of the tube. The other tube is just so much wider as to fit exactly over the former one; it has likewise, on one side, a large opening corresponding to the one in the narrower tube. By the wider tube being moved forwards or backwards, the point of the trocar may be covered or denuded, and in the same manner, the opening leading into the narrower tube is made larger or smaller, according to the desire of the operator. For the more accurate description of the instrument, we must refer the reader to the original paper.—*Illustrirte Med. Zeitung, Dr. Rubner, Muenchen*, 1852, No. 1.

Statistics of Fractures and Dislocations treated in the Pennsylvania Hospital for Ten Years.—By Dr. Norris.

Eighty-four dislocations in 10 years; viz., 52 of the shoulder, 4 of the hip, 2 of the astragalus, 9 of the elbow, 9 of the clavicle, 2 of the radius, 1 of fingers, 3 of thumb, 1 of knee (incomplete), and 1 of semilunar cartilage. 78 were cured, 5 removed, 1 died. Of the shoulder, 39 were dislocations into the axilla, and 10 were forward, under the clavicle.

In 12 years there were 27 compound fractures of the thigh, and 139 of the leg; 50 underwent amputation, of whom 20 died. 116 were not operated upon, and 51 died; 22 of the deaths occurred within 24 hours after the accidents. Of the whole number, 53 were from railway accidents.—*American Journal of Medical Science*, Oct. 1852.

On Adulteration of Sulphate of Quinine. By Dr. Moll.

The excessive price of the true cinchona, the calisaya of Bolivia, has led to the substitution of many inferior kinds, chiefly remarkable for their containing large proportions of *quinidine*. In consequence of their lower price they have obtained admission to the quinine manufactories in large quantities, and much of the sulphate now produced is depreciated by the addition of quinidine. This substance differs from the sulphate of quinine by its greater specific gravity and less flocculent crystallization, and it is much more soluble than it in water and alcohol. The addition of both *cinchonine* and *quinidine* may be detected by means of ether; for while cinchonine is almost insoluble in this substance, quinidine is so in a far less degree than is quinine, inasmuch as sixty drops of ether and twenty of ammonia will dissolve ten grains of quinine and only one grain of quinidine. On the addition of these quantities of sulphuric ether and liq. ammonia, to ten grains of quinine, with ten drops of dilute sulphuric acid, and fifteen of water, all will remain dissolved, unless cinchonine, or more than 10 per cent of quinidine, be present, the mechanical impurities only appearing at the surface. If 10 per cent. of quinidine be present in the ethereal solution, it will soon crystallize on the surface of the ether. Traces of this substance can be yet more certainly dis

: if ether saturated with quinidine be employed, when all that exists in the [?]ted salt will remain insoluble. If the powder contain cinchonine, or more [?]10 per cent. quinidine, it will remain undissolved at the line of demarca-[?] of the two fluids. If it be quinidine, it is soluble in additional ether, which [cin]chonine is not.

To establish the purity of quinine, we must also assure ourselves of the absence of *inorganic* substances, by calcination in platina, or by a solution of the salt in alcohol. Sulphate and carbonate of lime, magnesia, &c., remain undissolved, while boracic acid, though soluble, betrays itself by its blue flame on conflagration. The absence of *organic* substances, as [sal]icine, sugar, starch, stearic acid, is known by the colourless solution which takes place in concentrated sulphuric acid. The presence of *ammoniacal salts* is revealed by the odour which ensues on the addition of caustic alkali.—*Revue Medico-Chirurgicale*, xii. 238.

Experimental Inquiry concerning the question, whether the Purgative Action of the Neutral Salts is the effect of Endosmosis? By Dr. H. Aubert.

Aubert made his experiments with the purpose of examining the correctness of the view spread under *Liebig's* authority ('Untersuchung der Mineralquelle zu Soden und Bemerkungen ueber die Wirkung der Salze auf den Organismus.'—*Wiesbaden*, 1839), that the purgative action of the neutral salts is a merely physical process, being the consequence of exosmotical transudation from the walls of the intestinal tube, effected by the more concentrated solution of these salts within the cavity of the tube. The experiments were instituted in the following manner:—Solutions of different neutral salts were put into a cylindrical glass tube, the lower end of which was covered with a piece of membrane, from a pig's bladder, then the tube was immersed into serum of blood, and the changes going on in the solution within, and the serum without, were examined at different intervals. Another series of experiments was made by taking internally solutions of these salts. As the result of both series, Aubert draws the following inferences:

1. The purgative effect is not influenced by the degree of concentration of the solution, the number of stools produced by a certain quantity of salt will be the same, whether the salt is dissolved in six or seventy-two ounces, the water of the solution is excreted by the kidneys, the salt exercises its influence on the bowels. 2 No albumen is found in the alvine excretions, as ought to be the case, if the action of the salts was an endosmotical one. 3 The quantity of salt excreted through the urine, compared with the quantity of water contained in the discharge from the bowels, is not that which it ought to be, according to the laws of endosmosis and exosmosis (as much, at least, as they are known at present). 4 Peristaltic motion of the bowels is constantly excited by the neutral salts; the rolling and rumbling, which Aubert always observed soon after taking the salts, as well in a concentrated as when in a diluted solution, is attributed by him to the action of the salts on the nerves of the intestines, and to the reflex motion excited in consequence of this. 5. A part of the *sulphate of magnesia* appears to be decomposed within the organism, as the magnesia is excreted in a larger proportion than the sulphuric acid with the fæces, the sulphuric acid in a larger one than the magnesia with the urine 6. The remedy produces the characteristic effect on the bowels, when a solution of it is merely infused into the veins of an animal.

It must be, however, remarked here, that *Liebig* himself, in a work of a later date ('Untersuchungen ueber einige Ursachen der Saeftebewegung im thierischen Organismus,' 1848), states that he does not intend to explain the whole action of the neutral salts by endosmosis, but that he considers this to be one of their influencing qualities.—*Henle und Pfeufer's Zeitschr. fur Ration. Medic.*, 1852, Bd. ii. p. 225.

NOTE.

OUR correspondent who inquires respecting the composition of the 'Eau Pagliari,' is informed that it is the compound tincture of benzoin, which is used in its composition.

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